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ARTICLES

American Political Science Review Vol. 86, No. 3 September 1992

PEASANTS OR BANKERS?
THE AMERICAN ELECTORATE AND THE U.S. ECONOMY

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The usual model of electoral reaction to economic conditions assumes the "retrospective" economic voter who bases expectations solely on recent economic performance or personal economic experience (voter as "peasant"). A second model assumes a "sophisticated" economic voter who incorporates new information about the future into personal economic expectations (voter as "banker"). Using the components, both retrospective and prospective, of the Index of Consumer Sentiment (ICS) as intervening variables between economic conditions and approval, we find that the prospective component fully accounts for the presidential approval time series. With aggregate consumer expectations about long-term business conditions in the approval equation, neither the usual economic indicators not the other ICS components matter. Moreover, short-term changes in consumer expectations respond more to current forecasts than to the current economy. The qualitative result is a rational expectations outcome: the electorate anticipates the economic future and rewards or punishes the president for economic events before they happen.

Economics moves political behavior. With hard times, administrations lose support; with good times, they gain it. We know this to be true. But when we ask how—by what processes—the political translation of economic experience occurs, the answers are far less certain. We do not know how the electorate experiences movements in the domestic economy or even why it cares. After decades of attention, we are little beyond introspection in understanding the processes by which citizens come to perceive economic movements. We do not know how they generalize from that experience to political evaluations. We do not know how they reach politically relevant conclusions about the political–economic future from the experiences of yesterday and today.

Beyond the causal dynamics lie matters of deeper consequence. In particular, we are interested in the quality of intelligence that governs the translation of economic experience into politics. Consider two caricatures: peasant and banker. The peasant judges the government by present personal experience. He or she eschews abstraction and, instead, relies on what may be seen and felt directly, on direct personal experience. The future is imagination, the present is reality. Turning to politics, the question is simply put: "What have you done for me lately?" The banker, in contrast, is indifferent about the past except as it portends the future. The banker judges the government by its ability to shepherd the future. Ignoring current conditions, the banker attends to matters of systemic consequence that indicate the government's wisdom, rather than its appetite. The banker asks, "What are your prospects?"

Clearly, these are matters of degree, rather than absolutes. No individual—and certainly no society—will act purely as peasant or as banker. It is our intent to evaluate the extent to which the U.S. political economy reflects the intelligence of peasant or banker. We wish to see how far political judgments are driven by directly experienced personal economic conditions on the one hand, and by expectations about the nation's economic future on the other. The character of intelligence that dominates the political economy is of apparent import on its own. In addition, however, our understanding of it should contribute mightily to a more general theoretical view about how industrial democracies use information to govern themselves.

Understanding that citizen economic perceptions affect political judgments only raises the question, What sorts of economic information drives those subjective perceptions and thus drives politics? In particular, are the economics of politics based in a reality that people can directly experience, as measured in assessments of their family's current economic well-being or do the economics of politics reflect a more distant and abstract view of national conditions or even expectations about national conditions in the future? In the jargon of current theoretical debate, one central question is whether political evaluations are moved by "pocketbook," or by "sociotropic," economic perceptions. This question is intertwined with another: Are political evaluations moved by retrospective, or by prospective, economic evaluations?

In the political science literature, the model of the economic voter has undergone considerable evolution. In an earlier day, the voter was understood to respond to economic conditions; but the psychology of the response was not mapped much beyond the general notion of stimulus and response (see, e.g., Key 1966). Good economic fortune led the economic voter to reward the incumbent, and bad times led the economic voter to punish. Whether this response was instrumental or affective was rarely at issue. In the extreme, one could imagine the response of the economic voter as purely an emotional reaction in terms of anger or gratitude, void of any cognitions about the economic future. (On affective reactions to the economy, see Conover and Feldman 1986 and Peffley 1985).

Most contemporary discussions of economic voting treat economic voting as an instrumental act. As Fiorina (1981) has forcefully argued, the retrospective voter can use personal economic circumstances to form expectations about the voter's personal economic future under the current incumbent. In this sense, the retrospective voter is really prospective in nature. The economic voter may be a "peasant" in the sense of being guided by little more than personal experience but still use this limited information instrumentally as the best available guide to future economic reward.

As Kinder and Kiewiet (1979, 1981; Kiewiet 1983) have emphasized, voters often react to their perceptions of the national economy ("sociotropic voting"), rather than to personal economic circumstances. A sociotropic voter can use the current health of the economy as a signal of the incumbent's economic competence that will influence the voter's economic prosperity in the future. The sociotropic prospective voter meets some of the preconditions of the "banker" of our model.

Whether guided by their own pocketbooks or by their perceptions of national conditions, the behavior of instrumentally retrospective voters is roughly in accord with the economists' model of "adaptive expectations" (see Alt and Chrystal 1983). According to the adaptive expectations model, people modify their expectations about the future by extrapolating from a weighted average of current and recent values. An important limitation of this version of the economic voter, however, is that although prospectively oriented, the economic voter is totally myopic. Unable to look beyond current conditions, the myopic retrospective voter does not react to the future implications of current policy nor even to economic forecasts. Easy to fool, the myopic economic voter is a crucial ingredient in most models of the political business cycle (e.g., Nordhaus 1975; Tufte 1978).

In recent years, many economists have become attracted to "rational expectations" models of economic behavior. Without necessarily buying controversial "rational expectations" arguments about macroeconomic policy, we can imagine a "rational expectations" model of the economic voter. The heart of rational expectations economics is the notion that decision makers incorporate all available information, responding to events when they are anticipated, rather than waiting until they occur. Applied to economic voting, rational expectations would result in an electorate that responds to messages about the future economy, rather than extrapolates from current conditions. In the aggregate, we can imagine an electorate guided by the same intelligence as the economic forecasters. This electorate would discount the current economy, because current conditions (except for possible surprises) were built into previous forecasts. The rational expectations version of the economic voter will respond with little gratitude for past prosperity independent of future economic promise. In this sense, the rational expectations economic voter acts very much like a banker, rather than a peasant.[1]

MICRO ANALYSIS AND THE UNDERSTANDING OF ECONOMIC VOTING

The main evidence that the economy has important political consequences comes from macro analyses of both election outcomes and the presidential popularity time series. What we know of the psychology of the process, however, comes from the micro analysis of voter surveys. Two findings stand out in this literature: (1) voters respond more to their perceptions of the national economy (sociotropic voting) than to their personal financial experiences (pocketbook voting); (2) whether pocketbook or sociotropic, economic voting is strongly prospective, so that when surveys attempt to ascertain economic expectations independent of retrospective judgments, prospective expectations clearly drive out retrospective evaluations as predictors of vote choice (Kiewiet 1983; Kuklinski and West 1981; Lewis-Beck 1988).

But the findings of survey research are not at all tidy. For instance, prospective expectations appear to be only weakly connected to retrospective evaluations (Conover, Feldman, and Knight 1987; Kuklinski and West 1981). Except for extrapolation from current and recent conditions, we know little about the sources of peoples' economic expectations. Other identified sources of economic expectations include the individual's partisan bias and the persistence of past expectations (Conover, Feldman, and Knight 1987). Do citizens respond to economic forecasts? We know little about peoples' exposure to economic forecasts, although some scattered experimental evidence suggests that people do take forecasts into account when they are exposed to them (Ansolabehere, Iyengar, and Simon 1990; Olshavsky and Jaffee 1981).

Using survey research to draw inferences about the psychology of economic voting is made difficult by the limitations of micro analysis. For instance, as Kramer (1983) reminds us, very little micro change in family finances is government-induced. The result is a severe downward bias to estimates of the political response to government-induced changes in family economic circumstances. As Kramer also observes, given a survey cross-section, variations in evaluations of the economy can arise only from variations in perceptions, rather than variations in actual economic performance. These perceptions are prone to partisan rationalizations. A worrisome hypothesis is that respondents sometimes select opinions about the national economy or even their own pocketbook on the basis of consistency with their partisan views (Sears and Lau 1983; but see also Lewis-Beck 1986).

Partisan rationalization may be a special problem for direct evidence of prospective voting. Arguably, to ask respondents to make economic forecasts is to invite "doorstep" opinions, virtually made up on the spot. When asked in the context of a national political survey, respondents may be even more prone to manufacture economic forecasts from their vote choice than they are to bend their perceptions of current conditions in the direction of their vote choice. For this reason, micro survey evidence of prospective voting must be treated with particular caution.

CONSUMER SENTIMENT AND ITS COMPONENTS

The present study offers a time series analysis of the connections between (1) objective economic indicators, (2) aggregated economic cognitions, and (3) presidential approval. The economic cognitions are the Index of Consumer Sentiment (ICS) and, crucially, its components. The ICS and its components have been measured as part of the Survey of Consumer Finances and Survey of Consumer Attitudes and Behavior by the University of Michigan's Survey Research Center since 1953. The ICS is known to be responsive to the national economy (Katona 1964, 1975) and usefully augurs the economic future, as well (Fuhrer 1988; Matsusaka and Sbornone 1992). Its components have previously been shown to predict presidential approval (R. Shapiro and Conforto 1980). Of particular use here, the individual items comprising the ICS are similar in format to the economic items used in the National Election Studies. (See Lewis-Beck's [1985] innovative analysis of individual-level ICS data.) Exploiting the aggregate measures of these variables, we can learn something about the aggregate psychology by which the economy affects politics.

The ICS is comprised of six items. Citizens are asked to evaluate

1. current family finances "Would you say that you (and your family living there) are better off or worse off financially than you were a year ago?"
2. current business conditions (technically not a component of the index, but asked in virtually all Consumer Sentiment Surveys) "Would you say that at the present time business conditions are better or worse than they were a year ago?"
3. current buying conditions "Generally speaking, do you think now is a good or a bad time for people to buy major household items?"
4. next year family finances "Now looking ahead—do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?"
5. short-term business expectations "Now turning to business conditions in the country as a whole—do you think that during the next 12 months we'll have good times financially, or bad times or what?"
6. long-term business expectations "Looking ahead, which would you say is more likely—that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?"

The wording of the questions suggests clear-cut measures of pocketbook (items 1, 3, and 4) and sociotropic (2, 5, and 6), as well as retrospective (1–3) and prospective (4–6) evaluations. Indeed, the Survey Research Center combines items 4–6 into a separate Index of Consumer Expectations. Each item (and the index as a whole) is scored on a 200-point scale representing the net balance of positive and negative opinion, with 100 representing the neutral point.

Evaluation of current buying conditions (item 3) does not cluster with the other items and contributes little to our understanding of political attitudes. Also, the two measures of business expectations are too highly correlated (.95) to include both. When the two are in the same regression equation, the long-term measure always dominates short-term expectations. Accordingly, we drop the latter and keep the former. This leaves four aggregate measures of economic opinion: (1) mean perceptions of current family finances, or *Personal Retrospections;* (2) mean perceptions of current business conditions, or *Business Retrospections;* (3) mean perceptions of next year's family finances, or *Personal Expectations;* and (4) mean long-term *Business Expectations.*

These variables are measured on a quarterly basis since the fourth quarter of 1952. Except for occasional missing data, our data are an almost continuous series from the third quarter of 1954 to the second quarter of 1988. The paths of our four aggregate measures are highly correlated in our time series. On average, they correlate at .74 when measured concurrently and at .67 over one lag. Periods of prosperity lead to generalized good feelings while recessions affect both current and prospective evaluations of self and society. Yet the extent of this covariance is not overwhelming. The different series react differently to economic stimuli by taking different paths through time. (For a thorough demonstration, see Mebane 1988.)

With macro analysis, we examine the net responses of the national electorate, rather than individual voters. A compelling advantage of macro analysis is that idiosyncratic sources of variation in economic judgment cancel out. Judgments whether the economy will improve or falter, for example, may be too noisy for worthwhile analysis at the individual level. But their noise cancels out in the aggregate, to provide the powerful measure of collective judgments of the economic future. Moreover, these data from economic surveys are decidedly free of the problem of political response rationalization that plagues micro analysis. On balance, the aggregated time series data set we examine here offers an important degree of inferential leverage that individual-level survey evidence cannot provide. Thus, our work both complements and extends the existing individual-level analyses.

Accounting for presidential approval in terms of collective economic beliefs is only one part of the task at hand. We hope, in addition, to understand how collective economic beliefs result from economic reality. The relationships between measured economic conditions and political evaluations might behave oddly for at least two reasons.

First and most important, the objective macroeconomic measures may not be good indicators of how individuals subjectively experience prosperity and hardship. Previous work (e.g., Katona 1964, 1975; Strumpel, Schmiedeskamp, and Schwartz 1973) shows that consumer sentiment is largely, but not entirely, a function of present and past economic indicators. The *not entirely* is important because the Index is valued as an indicator of future macroeconomic movements (and is particularly good at forecasting downturns; but see H. Shapiro 1972). In addition, cross-sectional evidence indicates that citizens react to the economy in ways that bypass cognitions about economic aggregates (see, esp., Conover and Feldman 1986). We may ask if citizen economic evaluations affect political judgments in ways that would not be predicted by directly observable economic aggregates. Having direct measures of subjective experience will enable us to examine the political economic links more carefully.

Second (more perversely), it may be that the macroeconomy affects political performance in ways that do not depend directly on how citizens evaluate the economy. Citizens may translate a societal sense of well-being or malaise into politics without being conscious of its economic underpinnings. This is most likely to apply to long-run relationships. For example, a president afflicted with an economic malady might come to be seen as incompetent, and subsequent political or international events might be perceived in that light long after the economy has recovered; or a lucky incumbent might find just the opposite. Further, affairs may be strategic and thus complex. For example, vagaries in economic fortune may encourage (or discourage) dramatic political adventure and thus political success or failure.

A CAUSAL FRAMEWORK

Before considering more subtle questions, we need to clear up some elementary matters of causality. Thus, we turn our attention toward the causal framework that governs the relationship between economic conditions, economic perceptions, and presidential approval. We lay out the evidence on four theoretically crucial questions: (1) Do real world economic conditions shape economic perceptions? (2) Do both economic conditions and economic perceptions affect presidential approval? (3) Do economic perceptions affect presidential approval in a way that is independent of the experienced economy? and (4) Does approval color subsequent economic perceptions?

For these questions, we turn to an analytic technique tuned to answer causal inquiries, a standard Granger causality test. While the conceptual argument that justifies this procedure is subtle,[2] the test itself is simple. To see whether the objective economy causes economic perceptions, we regress current perceptions (say, retrospective views of personal well-being) on their own lagged values and on the lagged values of economic conditions (here, unemployment change and inflation).[3] Then, using a standard partial F-test, we test whether the coefficients associated with all the economic variables might be zero, that is, whether the objective economy might not affect subsequent perceptions. If this null hypothesis succeeds, we infer that economic conditions do not cause perceptions; if the null hypothesis fails, we infer that conditions may cause perceptions.

The essential results appear in Table 1. Each row in the table represents an estimation equation, and each column represents the potential causal effect of a set of variables. The p-values associated with the relevant F-tests appear under the potential causal variable and in the row identified with the dependent variable. Thus, we estimate a .00 likelihood that the objective economy does not affect Personal Retrospections. We conclude, without much surprise, that Economic Perceptions of all sorts are clearly caused by the real economy. (The equations under (1) produce p-values of .00, .00, .00, and .01). Similarly, examining straightforward links to presidential Approval, we find that both the real economy and Economic Perceptions affect presidential Approval (p-values of .05 and .00). Again, no headlines.

We may also see whether the real economy's effect on Approval channels completely through mass perceptions or whether the economy affects Approval in indirect, perhaps Byzantine, ways. Accordingly, in the section labeled (3), we test the causal effects of the real economy and of perceptions *while controlling for each other*. If the economy affects Approval through causal pathways other than perceptions, then the economic variables should pass the multivariate Granger test. With a collective p-value of .17, they do not. Presidential Approval responds to the economy only to the extent the economy alters public perceptions of the economy. Meanwhile, with a p-value of .00, economic perceptions clearly affect approval even when not caused by the objective economy. Thus, we make two elementary assertions about the causal web. First, the experienced economy affects Approval through subjective Economic Perceptions. More intriguing, those Economic Perceptions also affect Approval for reasons not evident in the objectively experienced economy. This second inference provides the stimulus for the rest of our theoretical tale.

Finally, we can eliminate a potentially complicating

TABLE 1

Granger Causality Tests (Probabilities of No Causal Effect)

	CAUSAL VARIABLES		
DEPENDENT VARIABLE	ECONOMIC CONDITIONS	ECONOMIC PERCEPTIONS	PRESIDENTIAL APPROVAL
(1)			
Personal retrospections	.00	—	—
Business retrospections	.00	—	—
Personal expectations	.00	—	—
Business expectations	.01	—	—
(2)			
Presidential approval	.05	—	—
Presidential approval	—	.00	—
(3)			
Presidential approval	.17	.00	—
(4)			
Personal retrospections	.00	—	.97
Business retrospections	.00	—	.16
Personal expectations	.00	—	.64
Business expectations	.02	—	.86

Notes: Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Each row represents an estimation equation. Entries are *p*-values for appropriate F-tests on block coefficient restrictions. Each estimates the probability that the set of candidate causal variables does not "Granger-cause" the dependent variable. For economic conditions, the joint test is for two items: change in unemployment rate and inflation rate. For economic perceptions, the joint test is for four items: personal and business retrospections and personal and business expectations. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5. Each equation includes two lag terms (two quarters) for the endogenous variable—to control for autoregression—and one lag term (one quarter) for the potential exogenous variables. The first two quarters for any administration are eliminated (for this Granger analysis only).

possibility. From cross-sectional analyses, we understand that individuals color their perceptions of the economy by their prior evaluations of the president—Reagan supporters, for example, being less likely to acknowledge the recession of 1982 and Reagan opponents being less likely to acknowledge the subsequent recovery. Further, individuals who think the current president a competent (or incompetent) policymaker might rationally use that judgement to foresee a promising (or disappointing) future. These phenomena, so strong in the cross-section, need not translate into our cross-temporal data;[4] they need testing. The equations (labeled (4)) test Approval's effects on Economic Perceptions by controlling for the objective economy. In fact, we discover that Approval does not shape Economic Perceptions (the relevant *p*-values are .97, .16, .64, and .86). These results make our task much easier. We may straightforwardly model relationships between the economy, Economic Perceptions, and presidential Approval in a simple recursive system. Both economic experience and other things (yet unknown) shape economic perceptions, which then affect presidential approval.

MODELING PRESIDENTIAL APPROVAL

For our time series analysis of presidential Approval, we model quarterly Approval as a function of lagged Approval (at quarter $t - 1$) plus current values of our economic variables of interest. To guard against spurious effects, our Approval equations also include a number of standard controls, for which we do not present the actual coefficients. These include dummy variables for presidential administrations and controls for Johnson's Vietnam War (troops in Vietnam), Watergate, the Iran hostage crisis, and a modest series of important events.[5]

This specification represents a distributed lag model, using the Koyck transformation, which has become a standard approach to the analysis of presidential approval (Beck 1992; Kiewiet and Rivers 1985; King 1989). Independent variables are hypothesized to affect the dependent variable immediately and then leave a residue that declines gradually over time. With the Koyck transformation, the lagged values of the dependent variable capture the effects of lagged independent variables. This allows the convenience of including only current values of independent variables in the equations. Although regression coefficients represent the effects of current values of variables on current Approval (controlling for lagged approval), the effects of the independent variables cumulate, at a rate determined by the autoregressive effect of lagged approval. Thus, the effect of any economic variable will resonate not only in the current quarter but also feed forward into the future. Similarly, the current approval level will represent the effects of both current and lagged values of the independent variables. Since lagged effects decay exponentially, current and recent values outweigh those more temporally distant.[6]

TABLE 2

Presidential Approval by Economic Conditions and Consumer Sentiment

INDEPENDENT VARIABLE	APPROVAL[a] (1)	APPROVAL[a] (2)
Approval_{t-1}		
b	.87 (.04)	.82 (.04)
p	.00	.00
Inflation_t		
b	−.39 (.13)	−.17 (.13)
p	.00	.55
$\text{Change in unemp.}_t$		
b	−1.51 (.74)	.62[b] (.91)
p	.04	.50
ICS_t		
b	—	.21 (.05)
p		.00
Comb. sign. infl. rate, unemp. change	.00	.36[b]
Adjusted R^2	.933	.941
N	126	117

Note: Standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5.

[a] Each column represents a separate regression equation, each with approval as the dependent variable.

[b] Test includes coefficient with wrong sign.

THE ECONOMY, CONSUMER SENTIMENT, AND PRESIDENTIAL APPROVAL

Confirming conventional wisdom, the standard economic indicators are related to presidential approval. This can be seen from Table 2. Column 1 shows coefficients for the inflation rate and the quarterly change (first difference) in unemployment level. These coefficients are jointly significant, as one might expect.[7] But this is not the full story. Table 2, column 2 introduces the composite ICS. The ICS has a decidedly significant impact on Approval. As anticipated from our earlier discussion of the Granger tests, introducing the ICS wipes out the "direct" contributions of the economic variables. The obvious inference is that Consumer Sentiment is an intervening variable between the objective economy and Approval. The changing economy affects presidential approval because it affects Consumer Sentiment. Clearly, the economy affects approval by affecting perceptions of the economy, which are captured by the Index. Our next task is to ascertain which of the indicators comprising the index are the crucial intervening variables.

Table 3 displays the results of a race among the four chosen ICS indicators, competing to predict approval. Column 1 considers the effect of Personal Retrospections, running alone except for the control variables. As the sole measure of Consumer Sentiment in the race, Personal Retrospections perform credibly, with a showing that passes the .000 level of statistical significance. Column 2 introduces Business Retrospections to compete with Personal Retrospections. Here, we can ask, Where a gap exists between collective economic satisfaction and perceptions of the economy, which one wins? The answer is clearly the latter, Business Retrospections. Business retrospections make a significant contribution ($p = .03$) where Personal Retrospections falter with a decisively weak .63 p-value. Thus, in the battle between the familiar pocketbook and sociotropic measures, now taken to the aggregate level, sociotropic wins: a president achieves greater popularity by having people think the economy is booming when it is not than by having people prosper but view the economy as sick.

Next we allow the crucial prospective measures to do their work. Table 3, column 3 enters Personal Expectations to compete with Business Retrospections. We see that this new entry in the race passes the test of statistical significance and undercuts the coefficient for Business Retrospections. A reasonable explanation is that citizens reward the president for current business prosperity because of the implication that they will personally benefit later. Finally, we turn to Business Expectations. Presumably, the reason people think current prosperity will help them personally is that current prosperity will lead to future general prosperity. Column 4 confirms this expectation. The newest entry now dominates our race: with Long-Term Business Expectations entered, it alone is a statistically significant predictor of presidential approval. What the president gains from the perception of current business conditions is of little political value unless it translates into a perception of future national prosperity. In other words, a president achieves greater popularity by convincing the public that a slack economy will improve than by showering the nation with a prosperity that people worry will disappear. Moreover, since the coefficient for Personal Expectations also dwindles to insignificance with the entry of Business Expectations, a collective expectation of future personal well-being does not help the president unless it is generally attributed to a prospering general economy.

Table 3, column 5 shows the coefficients when all four indicators are raced together. Again, the measure of Business Expectations wins. With four components entered, Business Expectations is the entry that most clearly survives a test of significance. Still, we should be wary of the possibility that other worthy variables lose the race due to bumping others (multicollinearity) and not because of a lack of pace (substantive insignificance). However, the other three indicators are not even collectively significant. As a further test, columns 6–8 race Business Expec-

TABLE 3

Presidential Approval by Consumer Sentiment's Components

INDEPENDENT VARIABLE	APPROVAL[a]									
	(1)	(2)	(3)	(4)	(5)[b]	(6)	(7)	(8)	(9)	(10)
Approval$_{t-1}$										
b	.85	.84	.83	.81	.82	.82	.80	.82	.82	.83
	(.04)	(.05)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.05)
p	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000
Pers. retrosp.$_t$										
b	.13	.03	—	—	−.06[c]	.02	—	—	—	—
	(.04)	(.06)			(.06)	(.04)				
p	.000	.63			.35	.70				
Bus. retrosp.$_t$										
b	—	.04	.02	.00	.01	—	.01	—	—	—
		(.02)	(.01)	(.02)	(.02)		(.02)			
p		.03	.09	.85	.47		.59			
Pers. expect.$_t$										
b	—	—	.20	.12	.16	—	—	.11	—	—
			(.06)	(.07)	(.08)			(.06)		
p			.002	.08	.05			.07		
Bus. expect.$_t$										
b	—	—	—	.11	.10	.14	.11	.16	.16	—
				(.04)	(.04)	(.04)	(.03)	(.03)	(.03)	
p				.01	.02	.000	.001	.000	.000	
Bus. expect.$_{t-1}$										
b	—	—	—	—	—	—	—	—	—	.16
										(.04)
p										.000
Change in bus. expect.$_t$										
b	—	—	—	—	—	—	—	—	—	.16
										(.04)
p										.000
Adjusted R^2	.932	.929	.936	.939	.939	.944	.938	.946	.945	.945
N	121	110	110	110	110	115	110	115	115	103

Note: Standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5.

[a] Each column represents a separate regression equation, each with approval as the dependent variable.

[b] Significance of Personal Retrospections, Business Retrospections, and Personal Expectations = .24.

[c] Test includes coefficient with wrong sign.

tations against each of its three competitors separately. In each instance, Business Expectations decisively wins the contest with a highly significant coefficient, while the competitor fails to achieve statistical significance. (The closest is Personal Expectations with a .07 *p*-value.)[8] Column 9 shows the coefficient for our victor, Business Expectations standing alone in the equation. For every point of change on this 200-point scale, approval moves about .15 of a point. Column 10 decomposes this result further into the effect of the lagged level of Business Expectations and the current change in Business Expectations. Both the long-term cumulation and the short-term innovation in Expectations are highly significant.[9]

Clearly, the reason presidential approval responds to the economy is that presidential approval responds to economic expectations. Controlling for Business Expectations, no other measure of economic sentiment directly affects Approval. Economic conditions affect presidential popularity only to the extent that economic conditions alter expectations of the economic future.

ACCOUNTING FOR EXPECTATIONS

Our next challenge is to offer a fuller accounting of the sources of Business Expectations. A beginning of an answer, we think, is that people are embedded in a rich system of social communication. The social communication network is full of information not only about the past and the present but also about the future. Any encounter with the nightly news will subject the viewer to the bullish or bearish views of politicians, Wall Streeters, or economic correspon-

TABLE 4

Explaining Components of Consumer Sentiment

INDEPENDENT VARIABLE	DEPENDENT VARIABLE^a			
	PERS. RETROSP. (1)	BUS. EXPECT. (2)	ECON. NEWS (3)	BUS. EXPECT. (4)
Inflation$_t$	−.76 (.19)***	−.80 (.29)**	−1.34 (.40)**	−.41 (.34)
Change in unempl.$_t$	−5.86 (1.85)**	−.23 (2.58)	−11.11 (5.02)*	5.15 (3.02)^b
Change in leading indic.$_t$	−.01 (.09)^b	.25 (.11)*	.91 (.19)***	.12 (.12)
Pers. retrosp.$_t$	—	—	—	.08 (.10)
Econ. news$_t$	—	—	—	.16 (.05)**
Pers. retrosp.$_{t-1}$	.62 (.06)***	—	—	—
Bus. expect.$_{t-1}$	—	.79 (.05)***	—	.74 (.05)***
Econ. news$_{t-1}$	—	—	.47 (.07)***	—
Sign. of infl., unempl.$_t$	.000	.018	.002	.020^b
Sign. of leading indic.$_t$	.872	.025	.000	.320
Adjusted R^2	.742	.883	.792	.889
N	126	110	101	106

Note: Entries are unstandardized regression coefficients; standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988.

[a] Each column represents a separate regression equation.

[b] Test includes coefficient with wrong sign.

*$p \le .05$.

**$p \le .01$.

***$p < .001$.

dents. The news stories tell of new developments, sometimes citing inflation or unemployment numbers but more often describing a war, drought, strike, currency fluctuation, or daily rise and fall of the stock market. Almost inevitably, such events are read as good or bad omens. While the particulars of such accounts may fail to register in the public's consciousness, our evidence suggests that the general sense of optimism or pessimism seeps through the system in politically important ways.

The experts who translate the economic numbers into good times and bad times attend to more than the standard news. In the business of looking ahead, they absorb news that is critical to professional forecasting but often of little direct interest to the general public. Their translations convey their sophisticated understanding to all. Without trying, the public is exposed to the best information about the economic future that exists. Merely by noting that most forecasters say good (bad) times are ahead, the public becomes subject to the causal influence of the professionals' more esoteric tools.

If this story fits the truth of the flow of economic news, it follows that we should find some of the innovations in expectations flowing from early indicators of the economic future, such as the Commerce Department's Index of Leading Economic Indicators. Leading Economic Indicators measure the economic winds that are not otherwise apparent from current measures such as unemployment and inflation. We do not assert that the public monitors the Leading Indicators directly, nor do we believe that the Index comprises the entirety of economic intelligence. Rather, changes in the Leading Indicators provide a rough and consistently available measure of shifting expert forecasts. When the Leading Indicators influence the forecasters, that influence should find its way into ordinary peoples' views.

We test this thesis with a series of regressions, shown in Table 4, modeling quarterly change in Business Expectations and (for comparison) other Consumer Sentiment components. On the right-hand side, these equations include (1) lagged levels of the dependent variable, (2) the usual suspects of unemployment change and the inflation rate, and (3) the annualized quarterly growth in the Index of Leading Indicators.[10] For a baseline, Table 4, column 1 presents the Personal Retrospections equation. Quarterly aggregate perceptions of the family pocketbook are strongly predicted from their lagged values plus unemployment and inflation. This makes common sense: unemployment means hard times, and inflation both increases transaction costs and sometimes marks real income declines. But since Leading Indicators supposedly tap the future economy, not the present, there is no theoretical reason why current change in Leading Indicators should affect current personal economic well-being. This expectation is borne out by the nonsignificant coefficient for leading indicators in column 1. Aggregate Personal Retrospections represent a measure of current national prosperity—experienced by individuals and uncontaminated by expert commentary.[11]

Compare this pattern with the equation for our crucial variable of Business Expectations in Table 4, column 2. In this equation, the contribution of inflation is significant, while that of unemployment change is not. Bouts of inflation, of course, increase individual economic uncertainty and thus reduce confidence in the future. More to our point, however, current unemployment is not seen as a harbinger of the long-term economic future. Leading indicators, meanwhile, show a statistically significant effect, just as our social communication model predicts.

We have argued that the electorate develops its economic expectations from the economic forecasts available in the mass media. To test this proposition more directly, we can exploit another item from the Survey of Consumer Attitudes that we have hitherto ignored. Most Consumer Sentiment Surveys include an item asking respondents' perceptions of recent economic news: "During the last few months, have you heard any favorable or unfavorable changes in business conditions? What did you hear?" Aggregated, this measure of Economic News is the net balance of positive versus negative news perceived about the economy during the quarter. The question has a retrospective flavor that should prompt responsiveness to reports of current conditions (people out of work or back on the job, inflation at the supermarket, etc.), as well as professional economic forecasts. Tims, Fan, and Freeman (1989) show that this measure, when aggregated, corresponds closely with the economic news reported in the press. The question is whether the economic news that people hear reflects projections about the future, as well as summaries of the present. Table 4, column 3 shows the Economic News equation. As expected, News reflects current inflation and unemployment change. Provocatively, though, Economic News is even more strongly responsive to changes in Leading Economic Indicators.

When people make judgments about the future economy, they must rely almost entirely on two sources of information: their personal experience and the available economic news. We now have a survey-based measure of each: Personal Retrospections and Economic News. Personal Retrospections measures the aggregate sense of personal economic well-being within the electorate. Economic News measures the direction of economic news that people report hearing. We can incorporate each as an independent variable in our equations predicting Business Expectations. The results are shown in Table 4, column 4.

As expected, Economic News makes a significant contribution to Business Expectations. Including Economic News removes the direct effect of Leading Indicators (compare col. 2): economic forecasts affect expectations by passing through the Economic News. Further, neither unemployment change (wrong sign) nor Personal Retrospections contribute to Business Expectations. Collectively, people do not seem to make a connection between their—or the nation's—current standard of living and the economic future. Instead, they judge the future based largely on what they are told in the mass media.[12]

The electorate acts as if it develops sophisticated expectations based on economic forecasts rather than current economic conditions. This follows because, in the final analysis, the electorate relies most on what is reported in the news. And the news reports what the future holds.

EXPECTATIONS AND THEIR REALIZATIONS

Presidential approval is a function of the electorate's collective expectations about the economic future. These expectations are formed, in large part, by what economists and commentators assert about the economic future, rather than what the electorate senses about current economic conditions. One implication of this result is that to the extent that economists' forecasts are correct, the future realization of economic conditions should predict presidential approval at the expense of current conditions. Does this conjecture stand up to the data?

Consider the contribution of (anticipated) future economic conditions on approval. As a base, recall that current conditions clearly affect current innovations in Approval. (For convenience, Table 5, column 1 repeats the equation with Approval as a function of current economic conditions from Table 2, column 1). Both current unemployment change and current inflation are significantly related to Approval. But note what happens when future ($t + 1$) values of unemployment and inflation are added (column 2). The two future measures are (collectively) highly significant, while the current collection is now nonsignificant. Finally, column 3 shows the equation with the economic variables measured for the next quarter alone. The estimated economic effects are stronger than from the standard equation predicting Approval from the current economy.[13] These results have an obvious and important explanation. The apparent reason that current conditions predict Approval is that current conditions help to forecast the future. With next quarter's economic conditions known, the current economy does not affect Approval. Conversely, the introduction of current conditions fails to diminish the future's importance for current Approval. It is the future, rather than the present, that matters most.

There is one particular irony to this result. Political scientists have searched long and hard for strong

TABLE 5

Business Expectations, Realizations, and Approval

INDEPENDENT VARIABLE	APPROVAL[a] (1)	(2)	(3)
Approval$_{t-1}$	.86 (.04)***	.87 (.04)***	.88 (.04)***
Inflation$_{t}$	−.38 (.13)**	−.11 (.16)	—
Change in unempl.$_{t}$	−1.51 (.74)*	−.97 (.88)	—
Inflation$_{t+1}$	—	−.32 (.16)*	−.34 (.13)*
Change in unempl.$_{t+1}$	—	−1.57 (.96)	−2.42 (.73)***
Sign. of infl., unempl.$_{t}$	.0002	.40	—
Sign. of infl.$_{t+1}$, unempl.$_{t+1}$	—	.017	.0000
Adjusted R^2	.933	.937	.937

Note: N = 126. Entries are unstandardized regression coefficients; standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5.

[a] Each column represents a separate regression equation, each with approval as the dependent variable.

*$p \le .05$.

**$p \le .01$.

***$p \le .001$.

evidence that the economy affects politics and found this strong evidence to be somewhat elusive. This search has even lead scholars to search beyond current effects to seek out possible economic effects with long lags. Some scholars have reported delayed effects that do not "kick in" until a delay of perhaps several months (e.g., Monroe 1978). Others have even claimed that when conservatively specified, the search turns up empty (e.g., Norpoth and Yantek 1983). We suggest that the search has looked in the wrong temporal direction. Instead of finding the source of current changes in presidential approval somewhere in the economic past, we should find it in the immediate economic future.

DISCUSSION: A DEMOCRACY OF BANKERS?

We began with the question, When it makes political judgments based on the handling of the economy, does the American electorate act more like a peasant or a banker? The answer now seems clear. The electorate responds with the sophistication of the banker, evaluating the president on the basis of an informed view of the nation's economic prospects, rather than its current standard of living.

Consider the difference between our new model and the traditional model of presidential approval. In conventional wisdom, current economic shocks affect approval immediately; and these effects decay over time. Alternatively, current Approval is a function of the accumulation of past and current economic shocks; that is, the most recent economic events have the greatest impact on current approval, but current Approval still shows the effects of past events. The typically strong coefficient for lagged Approval indicates that the response to economic events takes a while to decay.

The difference between this traditional model and ours is the nature of the economic shock. In the traditional model, the shock is implicitly assumed to be the *realization* of an economic change. In ours, the shock is, at least partly, the *anticipation*. The electorate is foresighted, rather than myopic. If the economy is rosy but with dark clouds on the horizon, the electorate responds to the clouds, not to the roses.

Understanding that expectations, rather than retrospections, lie at the core of political evaluations forces a new view of the political economy. When citizens are retrospective, their politics are grounded in reality—personally experienced or observed in others. When citizens act on expectations, they rely on an informed imagination. This transformation of the base of politics, from reality to imagination, suggests a serious reconsideration of the role that information—and information production—plays in the polity.

First, think about the potential for political actors to manipulate economic and political outcomes. We might imagine such perversities as a president's maintaining approval by continually convincing the electorate that prosperity is around the corner even as the economy actually declines or the reverse case of a president who is pilloried for constantly negative economic forecasts even as the economy continues to prosper.

We discount the prospects for scenarios such as these, for two reasons. First, if such practices exist, they would be manifest in the data. We would find periods, for instance, when Leading Indicators persistently mispredict subsequent prosperity or when Expectations persist in one direction even though their trailing Retrospections go the other. We find no such patterns. When one quarter's innovation in economic expectations is wrong in one direction, the next quarter's innovation is as likely to err in the opposite direction: mistakes are not consistent. As these innovations add up, the errors cancel out to produce an accumulation of expectations that closely match their accumulated realizations.

Second, it is quite possible that when the electorate modifies its approval in response to a forecast that later proves to be mistaken, the electorate will eventually correct its evaluation in accordance with reality. In other words, the electorate may respond both to the immediate surprise of revised forecasts and to the gradual surprise of errors in past forecasts. Although our analysis has concentrated on the former, we observe hints of the latter.[14] The spirit of rational expectations surely incorporates such self-correction.

Further, because the electorate is foresighted, rather than myopic, it is less easily fooled by the kinds of short-term budgetary strategies that supposedly give rise to a political business cycle (e.g., Nordhaus 1975; Tufte 1978). Recent work on the political business cycle, in fact, assumes that the electorate enjoys the necessary sophistication to take into account the motivations of political leaders (Alesina, Londregan, and Rosenthal 1990; Alesina and Rosenthal 1989; Rogoff 1990; Rogoff and Sibert 1988).

Beyond the question of strategic manipulation, we must understand that even our bankers are at the mercy of the quality of the available information. For the electorate to evaluate presidential performance properly, economic forecasts must be accurate and readily available. Our research suggests these conditions are met but that the hold may be fragile.

Intriguing is the possibility that the economic expectations that move presidential approval are partially self-fulfilling—as if consumer confidence boosts the economy.[15] When expectations are self-fulfilling, judgments based on those expectations will always appear to be sound even when they represent little more than fantasy. For the practical policymaker, self-fulfilling expectations are two-edged: it is possible to effect economic and political change by affecting expectations alone; it is also possible to be unable to effect real change when expectations discount current acts. (For a thoughtful discussion and evidence on the matter, see Alt 1991.) For the political theorist, the matter is more profound: if political judgments are self-sustaining, by what standard can one measure the wisdom of democratic decisions?

Although we have presented evidence of an impressive economic intelligence on the part of the U.S. electorate, it is worth repeating that this result depends on the powers of aggregation. As individuals, voters show no strong talent for economic forecasting. Massive biases and seemingly random errors swamp whatever accuracy is present in individual forecasts (Conover, Feldman, and Knight 1987). When these weak forecasts are aggregated, however, the noise cancels out to leave only a signal surviving. The process of aggregating information is not unlike the process that leads a large jury to reach an accurate verdict in situations when many individual jurors do not (Grofman and Owen 1986; Miller 1986).

Our results do suggest one reason to offer special praise for the individual qualities of American voters. This is the electorate's choice of economic signal to guide its political judgments. Instead of judging presidents retrospectively, based on economic conditions as they happen, voters respond prospectively to the likely economic future. While this evidence of prospective behavior is limited to the evaluations of the economy's health and presidential approval (and thus does not directly address matters of policy and voting), it suggests richer possibilities in the political system than are often assumed. When politicians know that people care about the future, they will act accordingly. Thus, while a myopic electorate produces short time horizons for politicians' policies, a foresighted electorate may reward policies the benefits of which extend beyond the next election. If this extrapolation from our findings proves true, we shall need to recast the conventional wisdom about the dynamics that link the electorate, politicians, and public policy.

The fact that the aggregate public listens to, and moves in accord with, an informed elite analysis empowers that public to make collective political judgments quite beyond the individual talents of its members. The demonstrated importance of rational expectations expands our view of political life. The standard notion of "What have you done for me lately?" is a mean sort of politics. To the extent that the mass public's political evaluations of the present depends on how the future is changed, we have a politics that resides less firmly on the ground of everyday life and much more resolutely in the spirit of the human imagination.

APPENDIX: AN ANALYSIS OF THE INNOVATIONS IN RETROSPECTIONS, EXPECTATIONS, AND APPROVAL

Our straightforward regression analysis (reported in Table 3) shows that expectations are important even when statistically controlling for current retrospections. The conclusion is that expectations, rather than retrospections, are the proximate influence on approval. Moreover, a further analysis suggests that retrospections are rather unimportant even as an indirect cause of approval. The ordinary least squares evidence (see Table 4) shows that business expectations, which help drive approval, are quite unresponsive to retrospections.

The bulk of our evidence comes from a fairly simple set of regressions on quarterly time series data. We may buttress our argument by examining a Vector Autoregression (VAR) model of the same series of retrospections, expectations, and approval. The VAR analysis complements our work because (1) it is based on a loose specification (it does not assume that the past gets translated into the future in a particular or simple way), and (2) it focuses on the variables' *innovations* (it isolates the effects of expectations that cannot be predicted by previous retrospections, previous expectations, or previous approval). We use the subjective psychological variables (rather than objective economic measures) so that the results will sort out the retrospections–expectations ordering. We want to know not the prior sources of economic information but, instead, whether people process economic information as retrospections or expectations. We wish to see whether the innovations in retrospections or in expectations drive approval.[16] We present a brief summary of the key results in Table A-1. (A fuller description is available on request.) Look first at column 1. The entries represent the proportion of variance in Approval that may be attributed to innovations in Approval, Personal Ret-

TABLE A-1

Variance Decomposition of Approval for Three-Variable Vector Autoregression Model

ENDOGENOUS VARIABLES	APPROVAL[a] (1)	(2)
Presidential approval	.27	.59
Personal retrospections	.15	—
Business retrospections	—	.12
Business expectations	.58	.29
N	96	80

Note: Data are quarterly, from the secord quarter of 1954 to the second quarter of 1988. These estimates are based on a three variable VAR. The system estimation equations include, as exogenous variables (not shown), terms representing political events, the Vietnam war, and dummy variable shocks for the first month of each administration. See n. 5. Each entry gives the proportion of variance that may be attributed to innovations in each of the endogenous variables. The estimates reflect the system state after it has "settled down"—here after eight quarters.

[a] Each column represents a separate VAR analysis. Column 1 includes presidential approval, personal retrospections, and business expectations as endogenous variables; column 2 includes presidential approval, business retrospections, and business expectations.

rospections, and Business Expectations.[17] We see that innovations in Personal Retrospections comprise 15% of the variance in Approval, while innovations in Business Expectations comprise 58%. Clearly, Business Expectations dominate. When we turn to Business Retrospections (col 2), the case is similar: retrospections and expectations generate 12% and 29% of Approval, respectively; that is, of the variance in presidential approval that stems from the economy, the major portion starts not as retrospections but, instead, as expectations.

It is thus clear that innovations in expectations, quite independent of experience, contribute mightily to Approval. To be sure, both retrospections and expectations matter: we do not wish to suggest that experience is irrelevant. Yet when people digest information about economics and apply it to politics, information about the future carries special weight.

Notes

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1. In the main, we are interested in matters of intelligence. We here wish to be careful about pushing the term *rational expectations* too far. We do *not* wish to test the public's sagacity against the economists' technical standard of "efficiency"; that is, we do not expect to find that the public uses *all* available information in its forecasts. Instead, we wish to return to the more commonsensical notion of expectations formation. By *rational* we simply mean that when guessing about the future, the public considers information beyond that embedded in immediate past experience. In politics, rather than economics, this is the crucial distinction. The plausibility of the electorate's offering political responses to rational expectations about the economy has been the subject of some interesting debate (see Alt and Chrystal 1983; Beck 1991; Chappell and Keech 1985; Chrystal and Peel 1986). We wish to join, redefine, and enrich, that debate.

2. In a philosophical sense, causality is a complicated matter. Granger tests discover whether a particular variable helps "predict" future manifestations when all other variables are controlled. Passing this test accords with commonplace views of causality. Yet our data include one clear case of finding "Granger causality" when causality is ambiguous. Without a doubt, the economic expectations data may be said to "Granger-cause" future unemployment. It is possible to believe that this is so, since consumers adjust their behavior, and producers (knowing consumer forecasts) adjust theirs. It is also possible that this causal mechanism is missing and that consumers merely properly forecast the next quarter's—or the next month's—economic climate. Granger tests are also sometimes insensitive to cointegrated causal relationships. We interpret them with that caution in mind. (For a clear exposition on causality tests in political science, see Freeman 1983.)

3. We include in these estimation equations, as in all others, (1) dummy variables for each presidential term, (2) a series of variables representing dramatic political events, (3) the number of troops in Vietnam (during the Johnson years). These additions sharpen estimates and correct for potential biases associated with underspecification. See n. 5. We have employed a short "lag window" for our tests. This decision reflects a trade-off between a need to control for complicated autoregressive schemes in the endogenous variable and a desire for statistical power in the hypotheses tests. Considerable experimentation indicates that the exogenous variables' effects can be summarized with a very short lag length—one term does the brunt of the work. Accordingly, we have included on the right-hand side of each equation the previous two quarters' readings for the "dependent" variable (to wipe out most of the autoregressive structure) and the previous quarter for the candidate "causal" variables (to avoid adding noise to the hypothesis test). In order to minimize the difficulties associated with presidential turnovers, we have had to ignore the data for each president's first two quarters (for this Granger analysis only). A more conventional causality test—one that includes lags over longer periods (allowing for a more complicated autoregressive schemes)—produces a pattern of results very similar with one exception: it appears that the "objective" economy does not "Granger-cause" subsequent approval. Such evidence has been used to support rational expectations hypotheses (Chrystal and Peel 1986). In the end, the debate centers around the allowable complexity of causal schemes. Rather than concentrate on the technical matters surrounding such tests, we here wish to look for hints of causality and to establish a plausible causal sequence.

4. On the perceptual rationalization side, understand that the psychic processes that link presidential approval and expectations are most likely to be associated with steady-state emotional attachments (such as partisanship), sentiments relatively unaffected by month-to-month fluctuations in presidential approval. On the rational expectations side, recall that we model shocks to approval, that is, factors outside of the immediate past history of approval and economy. Our results are consistent with an understanding that these changes in approval reflect transitory performances on the political stage—that they are not the sorts of changes that revise judgments about economic management.

5. We have chosen to focus on a quarterly time frame because the Survey of Consumers economic perceptions data were gathered only quarterly (at best) until 1978. After that time, we averaged the monthly readings into a single quarterly reading. (We have replicated our analyses for the 1978–88 period with the monthly data and found no surprises.) Presidential approval data are drawn from Edwards 1990. Quarterly measures of approval are the mean approval of all Gallup surveys conducted for the quarter. Our "events" series is relatively slim, since we sought to include only events that left a major impact on the quarter's average reading and not just the temporary spike of a transitory rally point.

Positive events (+1) include the Geneva Summit in the third quarter of 1955 55:3, Cuban missile crisis 62:4, Moscow treaty 72:2, Paris treaty 73:1, Mayaguez incident 75:2, Camp David Accords 78:4, assassination attempt 81:2, and Granada invasion 83:4. Two negative events (−1) are Nixon's pardon 74:4, and Iranscam 86:4. Also, quarters 73:2–73:4 are coded as −1 events due to Watergate; Carter's Iran crisis is coded 2 for 79:4, 1 if 80:1, and −1 if 81:2. The event "effects" in the Approval equations are in the range of 6 points and highly significant. In every case, we have been careful to exclude information from the *previous* presidential administration from the Approval prediction equations.

6. To be sure, this Koyck-type model is a bare-bones version of a class of dynamic models. Given our present purposes, it does the job well enough. Our analyses of more complicated models do not suggest dramatic changes in theoretical inference.

7. We also tried unemployment level and first difference in inflation rate, but these versions of the economic variables did not contribute. Neither did per capita income growth. This variable performed poorly in all specifications and was dropped from the presentation. See also Kiewiet and Rivers 1985.

8. The political impact of Personal Expectations seems largely derivative. See nn. 12 and 17. Our long-term measure of Business Expectations also decisively wins a race with the short-term measure of Business Expectations (item 5 from the ICS). When the two are raced together in an Approval equation, our long-term measure wins, in terms of coefficient size, .13 to .02, and in terms of significance, .03 to .56. We explored several other specifications, and the dominant role of Business Expectations survived each. Among other variations, we eliminated the first year of every presidential term, we eliminated the final quarter of each lame-duck presidency, we deleted lagged Approval, and we substituted two-stage least squares for ordinary least squares.

9. This simple result is important for technical reasons. This model, by incorporating a "level" and a "change" specification on the right-hand side, approximates a "cointegration" model. The results provide stronger evidence that the relationship between Business Expectations and Approval is not spurious. For a discussion of these matters, see Engle and Granger 1987; Engle, Hendry, and Richard 1983; and Hendry 1986.

10. We employ the version of the Index of Leading Indicators that was replaced in 1989. Unlike the version of the index now in use, this older version does not include any direct measurement of consumer expectations. Scores based on the 1989 revision of the index incorporate the Survey of Consumer Attitudes' Index of Consumer Expectations (Hertzberg and Beckman 1989). Therefore, this newest version of the leading indicators historical time series cannot be used here. The quarterly measure of leading indicators is here lagged two months. For instance, for the fourth quarter, we use months 8, 9, and 10. This is to make the relevant time frame of the Index of Leading Indicators comparable to the other quarterly indicators. The monthly Index of Leading Indicators is not public knowledge until late in the following month. Monthly readings of current indicators are also reported on a delayed basis, but their measurement supposedly represents the experiential basis of economic information. The Index of Leading Indicators is a proxy for available forecasts.

11. Recall that in our *aggregate* analysis, personal retrospections do not reflect the idiosyncratic personal factors (such as illness or promotion or a firm's failure) that dominate cross-sectional work. Aggregate Personal Retrospections average out the idiosyncratic; fluctuations represent changes in the nation's economic condition as experienced by individuals.

12. We can also insert Business Retrospections on the right-hand side of the Expectations equation, on the grounds that perceived current business conditions account for business expectations independent of Economic News. But, like Personal Retrospections, Business Conditions are decidedly nonsignificant in the Business Expectations equation. Even when objective indicators (unemployment change, inflation, leading indicators) are deleted from the equation, neither retrospective variable achieves a significant relationship with Business Expectations. This result is as if, collectively, the electorate does not connect the future to the present. Retrospections are correlated with expectations—but seemingly only because past expectations, which are slow to change, predict current conditions. Also investigated (but not shown in Table 4) are equations for Business Retrospections and Personal Expectations. The former variable is largely driven by unemployment and leading indicators, with inflation playing a lesser role. The latter is strongly affected by inflation, but not by unemployment or leading indicators. When added to these equations, economic news matters for Business Retrospections but not for Personal Expectations. Both Personal Retrospections and Business Expectations contribute greatly to the prediction of Personal Expectations.

13. We can perform the same tricks when substituting Personal Retrospections for unemployment and inflation. For instance, the effect of Personal Retrospections loses its significance if future ($t + 1$) Personal Retrospections are included. Future retrospections are a substitute for current expectations.

14. Note the error correction mechanism implicit in the last equation tested in Table 3, column 3. This matter, of course, is far from fully explored.

15. Economists have long debated the role of consumer expectations in predicting economic growth. The empirical demonstration of a causal link from expectations to economic growth faces a special challenge. This is because residual effects of consumer expectations—unaccounted for by economic "fundamentals"—may be an artifact of omitted variables. For recent claims that consumer sentiment either affects (or uniquely predicts) the economic future, see Fuhrer 1988 and Matsusaka and Sbornone 1992. Whether causal agents or not, consumer expectations are commonly thought to predict economic change. Witness the Commerce Department's inclusion, starting in 1989, of the Survey Research Center's Index of Consumer Expectations (of which our Business Expectations is a part) as a new component of its Index of leading indicators.

16. The VAR models each endogenous variable as a function of its own history, as well as of the history of the other variables. (Here we use a history of four quarters, since longer ranges would lose many cases to missing data.) The least squares machinery estimates the endogenous structural parameters. The "variance decomposition" numbers stem from analyzing those structural parameters to find out what portion of the systematic variance in each variable may be attributed to innovations in each of the endogenous variables (including previous innovations in the dependent variable itself). Here, to save space, we show the variance decomposition of Approval only. (For expositions on VAR analysis, see Sims 1980; Freeman, Williams, and Lin 1989.) The actual estimation equations also include Vietnam Troops and dummies for Watergate, Iran, and a set of events (as in the regression equations shown in the main text). In addition, they include a set of "shocks" for each administration's first quarter (for which full data exist). For statistical purposes, these variables are treated as exogenous to the system.

17. These results show the state of the implied system after eight quarters. The effects of innovations begin to settle down in about this time frame; that is, looking at the system after a substantially longer period will produce similar results. Because retrospections and expectations covary within a single quarter and contemporary covariance cannot produce information about causal order, it is technically necessary to identify the simultaneous equations (of within-quarter sequences) by assuming that retrospections cause expectations or vice versa. Table A-1 shows the results assuming that within the quarter, retrospections cause expectations. In these analyses (but not generally) the two different assumptions produce substantive results within one percentage point of each other. Finally, for completeness, we replicated the analysis, having Personal Expectations compete with Business Expectations. Again, innovations in Business Expectations

dominates Approval's variance (by 55%, compared to 12% for personal expectations). It appears that most of Personal Expectations' political implications are derivative.

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THE PRESIDENCY AND ORGANIZED INTERESTS: WHITE HOUSE PATTERNS OF INTEREST GROUP LIAISON

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Studies of the relationship between the presidency and organized interests generally focus on presidential assistants and their communications with the interest group community. I take a different perspective. Based on presidential strategic interests and choices illuminated for several administrations through interviews with White House officials, four kinds of interest group liaison *are identified: governing party, consensus building, outreach, and legitimization. These approaches are then empirically evaluated for the Reagan White House using interviews with Reagan's staff and the responses of several hundred interest group leaders to 1980 and 1985 surveys of national voluntary associations. Like the Carter administration after its first year, the Reagan White House initially emphasized "liaison as governing party" built on exclusive and programmatic ties to groups. A less activist legislative agenda and new circumstances later shifted the emphasis of the Reagan and Bush administrations to other forms of interest group liaison.*

President Jimmy Carter discovered early on that achieving legislative success would require more than assembling comprehensive solutions to the nation's problems and simply offering them to Congress on their merits, even when both houses were firmly controlled by his own party. At first Carter was stymied almost everywhere he sought congressional action. Not only did his administration initially lack an overall legislative strategy and a means for identifying priorities, according to a senior Carter adviser, "in the first year we didn't have Anne Wexler—she came in later—who was able to mobilize the public interest groups in support of proposals."[1]

Anne Wexler's arrival in the Carter White House in 1978 brought the much-needed skills of a political insider, adding strategic depth to the Carter senior staff. But her entree to the White House as assistant to the president for public liaison also signaled a dramatic shift in how the presidency would interact with the interest group community. The White House staff now granted Carter's major legislative initiatives a new lobbying strategy that combined direct communications with members of Congress and the promotion of indirect pressures on Congress by White House–constructed coalitions of organized interests active in congressional constituencies. This new approach was designed to fuse presidential and congressional perspectives by transforming the goals and resources of like-minded interest groups into the political assets of the White House.

A well-developed set of instrumental presidential relationships with interest groups is not, however, merely the residual product of one administration's legislative difficulties. Previous administrations had practiced (though at a less sophisticated level) the art of strategic group politics; and once firmly established by the Carter White House, such a group strategy appeared to be a feature of the Reagan administration's efforts to move its 1981 legislative agenda of budget and tax cuts. Substantive interest group interactions with the White House of one sort or another have thus become a significant feature of the modern presidency.

I investigate the character of the relationships between national membership organizations and the presidency, as well as the reasons the relationships vary both across administrations and within a single president's Executive Office. Others have begun to address these issues, focusing on the derivation and formalization of interactions between the presidency and organized interests (starting with Polsby 1978). These works typically concentrate on presidential assistants and the specific roles they have played in communicating with the interest group world.

I take a different tack. I first construct a model of presidential–interest group exchanges that is rooted in the political and institutional environment in which presidents serve, the strategic choices they make to achieve their political and programmatic goals, and their capacity to regulate their interactions with outside constituencies in a way that fulfills their strategic objectives. The model yields a typology representing four ideal types of White House liaison with interest groups, reflecting the inclusiveness and programmatic content of purposive presidential interactions with the interest group community. I then offer an empirical test of the typology for the Reagan administration, examining whether the actual relationships with the Executive Office of President (EOP) reported by the executive directors of several hundred national voluntary associations in 1980 and 1985 are consistent with the president's strategic intentions for interest group liaison. (For a discussion of these surveys, see Walker 1991, app. A.) Interviews with two former presidents of the United States and White House officials from several administrations round out both the development of the typology and the assessment of the Reagan and Bush years.

I conclude that while White House liaison with interest groups has grown dramatically since the presidency of FDR, only a fraction of national membership organizations enjoy an active and cooperative relationship with the White House. For the most part, the groups experiencing such involvement with the Reagan EOP fit the expectations of a presidential strategy of "liaison as governing party," mobilizing ideologically compatible groups to support the president's legislative program. The considerably restricted legislative agenda of the later Reagan years and early in George Bush's term, however, at least temporarily reduced the importance of this pattern of liaison and highlighted others.

A TYPOLOGY OF WHITE HOUSE LIAISON AND INTEREST GROUPS

For the sake of clarity, I distinguish between two terms for White House–sponsored interactions with interest groups. For the interactions of the overall institutional presidency with organized interests I employ the term *interest group liaison,* which encompasses all communications between representatives of the interest group community and the president or presidential staff serving in the various offices of the Executive Office of the President. I use the more specialized term *public liaison* to describe the assorted activities of the formal Office of Public Liaison (OPL), which since 1974 has had the explicit mandate of conferring with groups. Both types of liaison may involve varying presidential liaison strategies.

Crafting a model of presidential liaison with interest groups begins with the proposition that organized interests seek access to the president in order to advance their specific objectives. Nurturing at least the perception of influence with elected officials (especially the president) helps group leaders persuade their members of their effectiveness. As the modern presidency draws more policy issues directly into the White House domain, the opportunity for groups to secure explicit communications with the president or presidential surrogates becomes an increasingly precious commodity. Presidents thus have something of value to offer cooperative group leaders.

Presidents also want something in exchange. They desire group actions that aid in the fulfillment of presidential objectives. To manage such efficacious relationships with the interest group system, however, requires a White House establishment capable of orchestrating exchanges with groups that are conducive to promoting a particular president's interests. According to Terry Moe (1985), a similar logic explains the generic institutional development of the presidency. Moe notes that presidents introduce changes in the EOP—the institution over which they have the greatest control—to enhance the congruence between the support structures they inherit from their predecessors and their own incentives and resources to make executive agencies as responsive to them as possible (pp. 236–39). Presidents lack the authority, time, and resources needed to redesign entirely the organization of the EOP; but they can implement modifications that help them fulfill their political and programmatic objectives.

Presidential freedom of action with interest group liaison is partially restricted by the political costs incurred by violating the expectations of access associated with some groups and fostered by previous institutional arrangements. Nevertheless, as much as group representatives may press for entry to the Executive Office complex (and most will have an incentive to do so), the tight security surrounding the president, the capacity of presidents to pick and choose their objects of attention, and the formal decoupling of presidential campaigns from interest group money give presidents considerable strategic latitude in deciding whom to let in the door and whom to keep out (see my earlier work, Peterson 1992, 224–25; see also Greenwald 1977, 215; Polsby 154; Scholzman and Tierney 1986, 327).

Designing an interest group liaison strategy requires presidents to make choices along two distinct dimensions. First, presidents and their staffs must determine the *breadth* of relationships to be cultivated with the interest group system, both as a general rule and for specific political or policy episodes. Interest group liaison could bring groups of all types into the White House, or it could limit the contacts to organizations ideologically compatible with the goals of the administration. The breadth of White House interaction with groups, therefore, can be either *exclusive,* targeted at a select and fairly narrow range of politically congenial organizations, or *inclusive,* reflecting a more representative cross-section of the variegated interest group community.

Second, presidents and their staffs must determine the *substantive focus* of the interactions with interest groups, yielding, for the sake of simplicity, two possible approaches: a *programmatic focus* and a *representational focus.* This choice distinguishes between policy and political motivations for interest group liaison. An administration with a programmatic focus works with interest groups to secure the passage or implementation of its policy agenda. Particular organizations attract the solicitations of the White House because of the influence they potentially wield in the legislative arena. A representational focus, on the other hand, emphasizes an administration's desire to reinforce its political standing, either among the president's electoral coalition or among group representatives at large. Attention is devoted to groups whose members are from constituencies in which the administration wishes to enhance the president's image.

Joining the two dimensions of breadth and substantive focus of group interactions produces the typology of four ideal types of liaison depicted in Figure 1. These ideal types are just that—simplified caricatures that are not likely to obtain in pure form during any administration but can be used to describe general approaches to, and actual broad patterns of, interest group liaison exhibited by the White House.

FIGURE 1

Typology of White House Liaison with Interest Groups

		Breadth of Group Interactions	
		Exclusive	Inclusive
Substantive Focus of Group Interactions	Programmatic	Liaison as ***Governing Party***	Liaison as ***Consensus Building***
	Representational	Liaison as ***Outreach***	Liaison as ***Legitimization***

Actual presidential decisions about the breadth of relationships and their substantive focus rest on the institutional, political, and economic contexts facing each president and three sets of precursor choices presidents make to fulfill their objectives within the contextually defined constraints and opportunities (see Bond and Fleisher 1990; Edwards 1989; Mezey 1989; Peterson 1990; Rockman 1984; Simonton 1987). First, presidents must decide what they wish to accomplish, merging compatible objectives and resolving the trade-offs inherent in satisfying a multitude of objectives. Will they emphasize solidifying their political stature and reelectability or give first priority to programmatic accomplishments? Second, having settled on their objectives, presidents must organize the means for achieving their ends. What kinds of coalition formation strategies are best suited to the president's political or programmatic goals? Third, each president must select individuals to fill various positions in the administration, selecting appointments capable of fulfilling the primary goals of the administration while also accommodating the need to reward allies, pay political debts, and mollify defeated factions.

Reviewing situations in which each of the approaches to interest group liaison has been implemented by past presidents demonstrates how these approaches derive from the context of a particular president's tenure in office and reflect the political and organizational biases of the individual chief executive. Interest group liaison as *legitimization* (e.g., the organization-based complement of the White House Press Office's efforts to solidify the president's image and prestige among the general public) is closely associated with the initial motives for constituting the new Office of Public Liaison (OPL) during the Ford administration. The word *Public* here highlights the intention of employing interest group liaison to reach the broader public through the myriad groups to which much of the public belongs.

William Baroody, who sold the idea of a public liaison office to Gerald Ford and served as its first director, had a vision of liaison that was neither programmatic nor exclusive in character. President Ford's political stature and legitimacy as president were threatened by Ford's selection via appointment rather than election, Ford's pardon of Richard Nixon in the aftermath of the Watergate scandal, and the increasingly problematic policies for managing the Vietnam War and an economy in deep recession. As Ford put it to me, "my becoming president was totally different from any other of the thirty-seven that preceded me, so I took office under very unusual circumstances; and for the first four months, from August until January, it was primarily a holding action. . . . So it was unique, and I hope one never to be repeated." If Ford was to lead the nation free of the Watergate and Vietnam debacles, he needed to project an image that was inclusive and representational of a wide variety of interests in the United States.

Baroody planned to nurture the President's legitimacy by providing an open forum for communications between the White House and interests of all kinds (Kumar and Grossman 1986, 97). According to a member of the Ford public liaison staff:

> We had our own theory. We followed a different approach than the previous administrations, such as Colson in the Nixon administration, and the Anne Wexler approach in the Carter White House. . . . The Office of Public Liaison should not be a political office. *It should not be organized specifically to be a lobbying arm of the president.*

> . . . In the long run, it accomplished some of those goods, from the perspective of the President. [But] its primary function was to signify two-way communication—on the one hand, to communicate the President's position to various constituencies around the country; on the other, to have a route for those groups to communicate their ideas. (my emphasis)

The OPL generally avoided both legislative lobbying and involvement in the 1976 presidential election campaign, functioning more like a congressional caseworker (Copeland 1985, 26, 36). Given the fact that the economic and budgetary crises of the day led the President to avoid any new domestic policy initiatives, except in the area of energy policy, there was little need for programmatically based group ties (interviews with President Ford and his aides).

At other times, presidents have sought to establish fairly inclusive ties with the group system—but for programmatic rather than representational purposes. Examples of this interest group liaison as *consensus building* may be found in the early Johnson administration. Johnson was unquestionably programmatically inclined, perhaps even to the point of recklessness, especially during the 89th Congress when his own popularity was high and the Democrats controlled large majorities in both houses. He promoted innumerable policies and forced the largest domestic legislative agenda of the modern era on Congress without much consideration for the fine points of implementation and consequence (Peterson 1990, 95, 244). Although the mobilization of interest group coalitions by the White House was not as pervasive as in later administrations, Johnson's legislative program reflected consideration of constituency group desires, such as civil rights for black Americans, as well as "whether outside interests could be mobilized" (interviews with Johnson aides). As a senior domestic adviser to LBJ expressed it, "We were not above going to the Sierra Club and getting them mobilized, or going to various interest groups and getting them to help break up the [legislative] log jam."

Although Johnson would not shy away from confrontation, his preferred form of legislating highlighted harmony, not conflict. Diverse political perspectives were to be melded, not divided. Doris Kearns, reviewing LBJ's political life, linked this perspective to his approach to coalition building:

> He moved up in the world, but he never forgot the place where he had been born; he simply added new constituents to the ones he had originally served. Separate packages, separately designed for separate groups—this was the winning strategy as Johnson defined it. Thus Johnson built his career on a series of disparate layers; he added one incompatible constituency on top of another; he juxtaposed contradictory ideas without choosing between them. (1976, 391)

Johnson hoped to achieve the Great Society on a bed of broad political consensus. The special circumstances of Johnson's early years in the White House made that possible. As one domestic staffer commented, recalling the congruence between the White House and Capitol Hill that existed prior to the sharp divisions over Vietnam, "You know, the separation of powers is a phrase we have all heard. Well, at that time some of the separation got collapsed. It seemed we were all working on the same thing. We were mobilized together." And the programs themselves invited a consensus among disparate organized interests. Whether the policy involved housing, urban development, transportation, education, or any number of Great Society initiatives, the programs brought together otherwise potentially incompatible organizations representing all levels of government that received funds, public and private sector providers of goods and services, and program beneficiaries (Reischauer 1986). Until the economy soured and Vietnam polarized the nation, every group could have a piece of the action and be folded into the hybrid legislative coalitions, with few excluded on ideological grounds (see Redford and McCulley 1986).

The modern presidency is often portrayed as an institution where interests that are otherwise ignored have an opportunity to be heard. Some presidents have been particularly active, for example, in initiating redistributive programs intended to ease the plight of the poor, who do not figure prominently in the distributive politics so ingrained in much congressional decision making (Spitzer 1983). Interest group liaison can provide an avenue of access for similarly disaffected groups. A liaison strategy of *outreach* emphasizes providing representation to segments of the president's political coalition that otherwise lack close ties to the government establishment, sometimes to the point of bringing somewhat suspect "fringe" groups into the system. Instead of using the OPL as a means to build support for the president's legislative program, Midge Costanza, Jimmy Carter's first public liaison assistant, "was involved in symbolic interactions with groups, some of which, such as gay rights activists, had no previous White House access," hoping to make Carter's an "open presidency" (Kumar and Grossman 1984, 300; Pika 1985, 20). As Carter's legislative needs grew more pronounced, Costanza's approach drew less respect from the senior staff; and her office, figuratively and physically, moved further away from the President.

Finally, there is interest group liaison as *governing party*. Examining the staff structure surrounding the president, Hugh Heclo notices that modern presidents "did not consciously plan to create their own political parties. Yet that, in embryo, is what has come to exist in the White House. . . . If one were inventing a political party, these [White House offices] are exactly the types of offices at branch headquarters that one would want to create" (1981, 168–69). Many of the positions formally held by presidential aides are direct reflections of the growth of the interest group liaison function within the EOP, culminating institutionally in the establishment of the OPL. The White House comes to resemble a *governing* political party, however, only to the extent that its primary focus is that of partisan coalition building for the purpose of achieving programmatic goals.

Upon entering office, Jimmy Carter was none too fond of inviting "special interests" into the White House; indeed, he was quite disdainful of Washington politics in general and its embodiment in the rhythm of interest group lobbying (Kumar and Grossman 1984, 308; Jones 1988; Pika 1983, 316–17; Pika 1985, 19; Polsby 1983, 124–25). In those initial days of 1977, "Mr. Carter, not unlike President Nixon before him, evidently came to believe that he was a beneficiary of a system of governing genuinely free of mediation processes in the ordinary sense. He appeared to believe that contemporary circumstances sharply reduced the need to exhibit traditional presidential concerns with coalition building in order to govern" (Polsby 1983, 130).

"Contemporary circumstances," however, ultimately proved otherwise. While complaining that "when you don't measure up 100 percent to those so-called liberal groups [in the Democratic coalition], they demand a gallon of blood; there's no compromise with them," Carter realized that an important strategy for persuading Congress on legislation "is to bring in groups of potential supporters, and to let them use their influence on the member of Congress" (interview). As part of the cabinet and White House shake up that followed Carter's 1978 reassessment summit at Camp David, the OPL "was radically revised with the explicit purpose of serving" the legislative needs of the president (Pika 1985, 20, 26). It is not surprising that liaison as governing party finally emerged with considerable force during the Carter administration, once the "outsider" president had learned the lay of the political topography. Combining an ambitious legislative agenda with the increasing centrality of the presidency in politics and policymaking, the decentralization of Congress, the apparently diminished utility of institutional partisan ties, the expanding universe of interest groups, and a nation still polarized after civil rights battles, Vietnam, and lowered economic expectations, the emphasis on exclusive and programmatic liaison with groups was consonant with the goal of forging coalitions in a world of political disharmony and fragmented power.

Carter's public liaison staff soon became involved in every presidential-level issue; and Anne Wexler, the new OPL director, immediately set about to "lobby the lobbyists. . . to get a large constituency on an issue" important to the administration (interview with Carter aide; quote from Kumar and Grossman 1984, 302). Where active support could not be won, "we worked very hard to neutralize" likely opponents, such as the business community on synthetic fuels (interview with OPL staff member). Wexler herself often chaired the elaborate task forces assembled to organize and manage the campaigns to support Carter's major policy initiatives. The task forces typically included people from the policy, congressional liaison, press, intergovernmental relations, and political offices, both from the White House and the relevant executive branch agencies. Public liaison, as an instrument of interest group liaison, could not have been more programmatically oriented.

In the extreme, the involvement of the Carter White House with interest groups achieved almost "corporatist" proportions. An elaborate process of group consultation was initiated for budget development, as described by a senior Carter aide:

> We set up a task force with people from the OPL staff, the Domestic Policy Staff, and [Office of Management and Budget]. We then brought groups in, either together or alone. We'd ask them what their priorities were, and their justifications for their priorities. Reports would be sent to Wexler, [Domestic Policy Staff Director] Eizenstadt, and to [OMB Director] McIntyre, balancing the interest group politics, on the one hand, with the President's needs. After the budget had been tentatively determined, we brought them in again. We tried out some trial balloons to see what the problems were. McIntyre liked it so much the first year that he wanted to do it again in '81. For the budget director, there is a real paranoia that the budget numbers will get leaked. Well, leaks always occur; but they didn't have any impact. The groups backed the budget structure, and they already knew what was in the budget, so the leaks did not have a negative impact.

The group representatives were more ready to support the president's budget, even with some sacrifices, as a result of being brought into the process. Such was the state of interest group liaison at the close of the 1970s.

I shall now take this thematic approach and evaluate the interactions of the White House with the interest group system during the Reagan presidency. Two questions are considered: What does the contextual and strategic situation that greeted President Reagan suggest about the administration's likely primary approach to the interest group community? Are our expectations about Reagan's interest group liaison strategies (which are generated from the president's vantage point) confirmed by the actual interactions that took place between the White House and interest groups, as characterized from the vantage point of group leaders?

THE WHITE HOUSE AND THE REAGAN COALITION(S)

Ronald Reagan was the first Republican president in a generation to assume the formal powers of the presidency at a time when their reach could be augmented by a confluence of favorable political forces. The Democratic opposition was in disarray, the Senate passed from almost three decades of Democratic hegemony to Republican control, enough Republican candidates swept into office to make the House of Representatives subject to the will of a conservative majority, and the Republican party itself stood unified at the ramparts, ready to do battle with the "statist" legacy of the dispirited Democrats. But unlike Dwight Eisenhower, the last GOP president similarly favored, Reagan assumed office armed with a simple but far-reaching programmatic agenda directed specifically at the institutionalized canon of the opposition party. The political environment in which Reagan entered office and the character of the goals he wished to advance had important implications for how his administration managed its relations with the interest group community.

Reagan and Liaison as Governing Party

In his approach to Congress, the courts, and virtually all aspects of politics, Ronald Reagan was arguably the most partisan president in modern American history. This partisanship extended to his perceptions of the interest group system, as well, leading to a two-pronged political strategy of crippling opponents and organizing the sympathetic. The administration launched an unprecedented and not entirely ineffective attack on interest group organizations that it viewed as both anathema to the interests of Reagan's policy agenda and representative of the inordinate influence the "liberal" establishment enjoyed in government (Peterson 1992; Peterson and Walker 1986; Wolman and Teitelbaum 1984).

For the second part of the administration's strategy, I explore the hypothesis that the Reagan White House also sought to achieve its primarily policy-directed objectives by securing the assistance of groups whose goals were compatible with those of the administration. The strategic position and purposive nature of the administration should have led to relations with interest groups that were exclusive and programmatically defined or reflective of interest group liaison as governing party. Since the aim of this kind of coalition building is to reinforce the president's political leverage over legislative coalitions in Congress, the White House should have sought to develop ties with sympathetic groups that had access to, and could influence, members of Congress through their districts. As components of an ersatz governing party, these groups linked the president and potentially allied representatives and senators by fortifying their policy convergence with the stimulus of real or threatened constituency pressure.

If this is a reasonable characterization of how the Reagan White House sought to engage the group system, then it should be manifest in the scope and nature of group involvement with the White House as retrospectively reported by the leaders of the membership organizations included in this study. Operationally, I hypothesize that four group characteristics were in evidence: (1) the White House interacted on a regular and cooperative basis with only a relatively small proportion of the available groups; (2) these select organizations expressed ideological positions on policy issues that were generally supportive of the goals of the administration and viewed the objectives of such a partisan president as relevant to their own concerns; (3) they possessed the kinds of organizational resources that made them productive members of a legislative coalition and were large enough—and endowed with sufficient and appropriate staff resources—to have a significant political impact; (4) they were politically active and motivated in ways that were helpful to the administration—specifically, giving some priority to legislative lobbying and perhaps indicating a willingness to be involved in electoral politics that would affect an organization's claim to access to a member of Congress. From the point of view of the White House, therefore, an ideal group to join the administration in liaison as governing party would have been a conservative organization, comfortable with the partisan orientation of the Reagan presidency, large and well staffed with individuals knowledgeable about legislative affairs, able to mobilize constituencies, and experienced in the art of lobbying the Hill.[2]

In the 1985 survey of national membership associations, group executives were asked, in separate questions, to "indicate whether this association communicates, consults, or interacts . . . frequently, occasionally, seldom, or almost never" with the OPL, other presidential assistants in the White House Office, the OMB, or any other office or agency within the EOP. From the responses to the four questions, I created a summary scale incorporating the highest frequency of interaction with any component of the EOP reported by each group. My interest here is not the average level of interaction between membership associations and the White House but, rather, the full extent to which at least some part of the presumably strategically coordinated White House operation penetrated the interest group world.

The Reagan White House, in fact, had regular, ongoing interactions with a rather limited number of groups in the sample of membership organizations. Only 11% of the groups reported frequent interactions with any part of the EOP. Moreover, just 8% of the associations perceived themselves as enjoying both frequent and "normally" cooperative communications with the White House. A third of the associations had no relationship at all with the White House, and many of the organizations experiencing some interactions found that the White House was "almost never" cooperative. This begins to confirm the anticipated exclusiveness of the relationship between the Reagan White House and the interest group community and also suggests that the modern presidency is not overwhelmed with interest group solicitors. That is not to say that the task before the White House staff was a simple one. Almost half of the organizations in the sample had at least occasional contact with the EOP, which translates into a considerable number of group representatives communicating with White House officials at some point during the course of the administration. That contact may be no more, however, than attendance at large, formal briefing sessions for scores of groups at a time.

The next issue is to determine whether the relative exclusivity of Reagan's communications with group leaders embodies the far more important characteristic of being related to the ideology and partisan standing of the groups. Is it the case that the more conservative a group's policy positions, the more likely it was to work with the White House? The simple answer is a clear *yes*. Two-thirds of the groups who favored the most conservative position on the provision of federal services had at least occasional interactions with the Reagan White House; fewer than a quarter of the most liberal associations had such access. A similar pattern is found when groups are categorized according to their views on federal regulation.[3] Only 4% of the groups with very liberal views on the provision of federal services enjoyed frequent access to the EOP, compared to 28% of the very conservative organizations.

Another way to assess the ideological and partisan sensitivity of the relationship between the Reagan White House and the group system is to isolate the groups that both expressed fairly clear ideological positions and observed differences between the two political parties relevant to their own concerns. Using those two attributes, about a third of the sample can be labeled "party identifiers," based either on their views about the provision of federal services or their positions on federal regulation.[4] From this perspective there is no doubt about the link between partisan positions and relations with the White House. (The Gamma measure of association is .69 based on federal services, and .66 using federal regulation.) In the case of party identification based on federal services, 94% of the groups with virtually no White House contact are "Democratic." Of the groups with frequent interactions 61% are "Republican" organizations, a particularly dramatic figure, given the overwhelming proportion of "Democratic" groups on these issues. At the extreme, only 5% of the "Democratic" organizations experienced both frequent and normally cooperative interactions. For the "Republicans," a quarter reported such relationships; and none was either ignored by, or had uncooperative exchanges with, the White House. The pattern is generally the same for the partisan divisions based on regulatory policies: 88% of those enjoying frequent interactions with the White House were "Republican" in their orientation.

Sharing the policy positions of the administration does not necessarily make an interest group much of an asset to a programmatically inclined White House unless it is also well positioned to contribute to coalition building in Congress. Cheerleaders are always appreciated and may be rewarded with a few White House courtesies, but small, inconsequential groups are not as likely to be selected from the myriad of contenders for regular presidential attention. Even among friends, politics is often quid pro quo. To fortify its leadership role on the Hill, therefore, the Reagan White House, to the extent that it engaged in liaison as governing party, should have emphasized its relationships with groups that possessed significant resources applicable to influencing the legislative process. In some cases, that may have meant working with organizations less ideologically pure but nonetheless willing to support the administration on particular issues.

One of the most important resources available to an interest group are the staff members who constitute the working core of the organization. It is the staff that meets with White House officials, mobilizes the group's membership, and communicates with members of Congress. Larger staffs are also likely to exhibit more specialization, including among their ranks those with political and legislative experience. The probabilities are also enhanced that the group will possess more organizational slack not needed for its own maintenance and that can be committed to issues relevant to the president's coalition. Group executives responding to the surveys also tended to report more complete information about the number of staff members than about either the magnitude of their budgets or the size of their memberships.[5] The size of an organization's staff, therefore, can be treated as a surrogate for a variety of resources that would be significant in any calculus that the White House would use to evaluate the utility of groups in a coalition.

The 1985 survey reveals that staff resources affected interest group liaison independent of group ideology, although the effects are most pronounced for moderate and conservative groups. The tau-B values between resources and interactions with the EOP for conservative, moderate, and liberal groups are .22, .30, and .07, respectively.[6] Groups reporting the most frequent contact with the Reagan White House tended to be those with large staffs—over 70% for the conservative and moderate groups and 55% for the liberal groups.[7] Further, groups whose personnel had previous legislative experience typically indicated more regular access to the White House than those without such staffs. Fully half of the conservative groups communicating frequently with the EOP possessed large and legislatively experienced staffs; 51% of conservative associations that "almost never" interacted with the White House had both small and legislatively inexperienced staffs. Over 80% of the resource-rich conservative groups gained at least occasional access to the White House. And of all the organizations interacting frequently with the EOP, one-half boasted large, legislatively experienced staffs. Almost 60% of those were conservative groups by any measure, and only 10% were liberal.

Finally, for liaison as governing party, driven as it is by the programmatic needs of the president, interest groups devoid of a concern for influencing the legislative process, or prevented by law from congressional lobbying, are of little use to the White House. Therefore, Reagan and his aides should have initiated contact more frequently with the groups that gave a high priority to working with Congress in the development and enactment of legislation (or for the defeat of unfavorable bills). In addition, the impact of lobbying activity can be enhanced if groups have been willing or able to participate in the electoral process, seeking to assist legislative candidates with compatible political interests.[8]

The groups in the sample indicated on six-point scales how important (least to most) among their activities the lobbying of Congress and campaign involvement were. Although these activity scales and the EOP interaction scale are not interval measures, I am assuming that they are sufficiently close approximations to permit the use of interval-level analysis and the reporting of Pearson-r correlation coefficients. The priority given legislative lobbying has a strong independent effect on the frequency of interaction with the White House for groups in each ideological category, with the values of r ranging from .37 for liberal groups to .54 for moderate organizations, with conservative associations in the middle at .42. Liberal groups may have claimed special consideration from the Reagan White House if their links to Capital Hill gave them some political purchase on the right issues. Campaign activity also demonstrated some impact, although it was considerably less than that of lobbying.[9] The values of r rest near zero (.03) for liberal groups and rise to only .20 for conservative outfits. It is not surprising, of course, that electoral pursuits did not advantage any liberal groups. Certainly, the Reagan administration looked askance at the notion of dealing too intimately with organizations that committed considerable time and resources to defeating the very members of Congress who gave President Reagan the influence he savored in Congress.

Other Forms of Interest Group Liaison in the Reagan White House

As depicted by the leaders of hundreds of national membership organizations, the relationship between the interest group system and the Reagan White House conformed in many respects to the dictates of liaison as governing party. If that approach to interest group liaison provided a *complete* picture of the link between groups and the White House, however, the surveys would not have also revealed the rather frequent communications with the EOP enjoyed by a set of liberal groups in the sample and by some of the conservative groups lacking resources or legislative relevancy. Quite clearly (as must be true of any administration in the modern era), the White House simultaneously engaged in other forms of interest group liaison. In the case of the Reagan White House, one can demonstrate that these were primarily liaison as legitimization and as outreach, each the respective product of two distinct causes: the evolution of the institutional structure and traditions within the EOP and the ramifications of personnel choices.

No matter what a president may wish to accomplish with interest group liaison, and no matter what general types of groups the White House would like to prevent from intruding on the internal processes of the executive branch, institution building and the emergence of political traditions conspire to deny the president complete flexibility when it comes to shaping and implementing a strategy of interest group liaison (see Moe 1985). Ever since aspects of interest group liaison became formalized in offices and staff positions in the EOP, certain categories of groups have benefited from specific representation in the White House establishment (Pika 1983, 318). For example, largely carried over from previous administrations, early in the Reagan administration specific White House aides had full-time, official responsibility for business, labor, and consumer groups, while informal ties linked the staff to groups representing women, blacks, Hispanics, ethnic Americans, the New Right, and the Jewish community. Similar but somewhat less intense attention was focused on the elderly, youth, and veterans (Pika 1982, 49). It does not take much imagination to understand that most of these "constituent" groups frequently have taken policy positions consistent with "liberalism" and that were contrary to the objectives of the Reagan administration. This legacy of institutional arrangements—too politically sensitive to challenge outright—gave some liberal groups access to the White House. Observing this pattern, Joseph Pika suggests that despite the recognized coalitional differences between the Republican and Democratic parties, when it comes to formal *public* liaison in the White House, "the similarities across party lines are striking" (1985, 6–7).

The presence on the White House staff of an individual with a portfolio of certain groups, however, says nothing about the *quality* of interactions those groups actually experienced. Only 44% of the liberal groups with frequent or occasional contact with the Reagan White House described those interactions as being "normally cooperative," compared to 63% for conservative groups. The panel data set provides further evidence of less-than-harmonious exchanges. The 1980 survey incorporated distinctions among groups active in several policy areas or representing various constituencies, including aging policy, as well as women, blacks, and Hispanics—all groups with special institutional access to the White House (number of cases = 49). Their reported frequency of interaction with the White House in 1985 is no different from the 1985 sample as a whole, despite the formal staff responsibilities for them. More importantly, of organizations with frequent or occasional contact with the EOP, only 38% found the interactions to be "normally cooperative."

The inclusion of some of the conservative groups in White House liaison efforts, on the other hand, highlights the importance of personnel decisions. Several of these groups gained entry to the White House without possessing the traits that would make them efficacious coalition partners. Their favored position resulted from the outreach efforts associated with particular individuals assigned to the White House staff; and this approach to liaison may have resulted from a substantively unrelated personnel selection process. According to a journalist who covered the Reagan White House, the OPL during Reagan's first year included hard-core conservatives whose views on liaison were contrary to those of Elizabeth Dole, the director of the Office. In an interview, he made the following argument: "I think she (Dole) found herself in a funny position, with no power whatsoever. She [was] not allowed to hire her own staff, and she didn't trust or like them. They are all right-wingers. She felt she was a prisoner of her staff." These staff members placed a higher priority than others in the White House on granting access to groups on the "Right," regardless of their legislative utility, much as Midge Costanza during the Carter administration reached out to groups on the Left without weighing what contribution they could make to moving the president's program on the Hill.

Dole's departure to become secretary of transportation and her replacement by Faith Whittlesey and then Linda Chavez only accentuated the tendency of the OPL to view itself as the focal point for the advancement of right-wing issues and the accommodation of conservative organizations. One member of the Legislative Strategy Group (LSG, the core assembly of senior White House aides in the first term concerned with legislative policy development and enactment) suggested that Whittlesey, due to her ideological fervor, was less effective than Dole. As he put it, "The joke in the White House right now about Faith is, 'You want to talk to me about budget cuts, I'll talk about Central America. You want to talk to me about the tax bill, I'll talk about school prayer'. . . . As a result, she's just not in the power structure in the White House, and she is not very effective." Whittlesey may have remained out of the policy loop, but she could still offer an ear to the interest groups that shared the intensity of her views. The outreach approach taken by the public liaison staff, while serving other purposes for the president, complicated the search for signs of an overall pattern of interest group liaison as governing party.

Having argued that at least three forms of liaison were practiced by the Reagan White House, the question arises as to whether these different approaches to interest group liaison can be traced to separate units within the EOP. In the Carter White House, the OPL under Wexler was central to organizing support for the president's program, coordinating the governing party functions of liaison while others performed outreach or legitimization. Under Carter, the central features of interest group liaison were, in fact, accomplished by the formal public liaison staff. The legislative role of the OPL in the Reagan administration, however, was tarnished throughout Reagan's time in office.

When one examines the levels of interaction between groups in the 1985 survey and various components of the Reagan Executive Office structure, one fact is immediately apparent: the OPL—*the official agency designed to handle interest group relations with the president*—was no more likely to have contact with groups than were the other offices within the EOP. The busiest staff groups in the White House Office were those outside of the OPL. These individuals would include the chief of staff; presidential counsels; congressional liaison staff; the policy development, political affairs, and communications offices; and so on. Many were apparently engaged in extensive communications with programmatically effective and politically compatible organizations. Unexpectedly, the survey reveals that the OMB had as much contact with interest organizations as did the public liaison operation, undoubtedly a sign of OMB's central role in the development of domestic policy during Reagan's first term (Benda and Levine 1988; Stockman 1986).

Why was the role of the OPL so circumscribed? The earlier discussion about its ideological temper offers one explanation. Another stems from the idiosyncratic situation that characterized the initial year of the Reagan administration and the original organization of the White House. Not only was Elizabeth Dole weakened by her staff, she was also handicapped by the vicissitudes of a mixed personal and political life produced by her marriage to Senator Robert Dole. As the previously quoted member of the LSG pointed out in our interview,

> In 1981 on the tax bill, one of the major players was Bob Dole. OPL was not part of the LSG meetings. Elizabeth Dole was head of the office, and we were not going to give our strategies away or we weren't going to complain about Dole with Elizabeth there, talking about Bob doing this or Bob doing that. . . . [As a result of that and the previously discussed ideological problems,] with the exception of the right-wing issues in '81, that office was totally ineffective. With the business community in 1981, several of us in the White House were involved. On the tuition tax credits, that office was very important; but if you want to talk about the major issues, it was others of us in the White House.

As a result, according to a senior adviser, the OPL "was given its directions by Baker, [secretary of the cabinet Craig] Fuller, or [congressional liaison official] Ken Duberstein."

It is therefore not surprising that the non-OPL part of the White House Office was even more actively involved in liaison with groups than the OPL itself—and often for more programmatic purposes. One Reagan aide commented that Max Friedersdorf, head of the congressional liaison operation, "went to almost more meetings with outside groups [than] he did with the Congress." It also conforms with John Kessel's examination of the influence structures in the Carter and Reagan White Houses, in which he concludes that Wexler gave strength to OPL, while Dole, unable to overcome the institutional weakness of the liaison office, "was a nonperson on the Reagan White House staff" (1983, 459; Kessel 1984, 251).

Certainly, the public liaison office contributed to pursuing an overall strategy of interest group liaison as governing party; and the senior officials in the White House used the resources of the OPL to help organize group events and communications to advance programmatic objectives. But the non-OPL staff also had available to them a set of liaison resources outside of the OPL that facilitated their involvement in a policy-based interest group strategy, such as the mobilizing strength of the Chamber of Commerce (until relations soured after 1981) and the "steering committee" established among business associations that coordinated their support of the administration (interview with White House journalist; Kumar and Grossman 1986, 98–99).

These overall distinctions among the *relative* roles played by various units within the EOP are also apparent in the kinds of groups with which they interacted, though these differences are muted by the complex imbrication of group relations with the White House. Almost every White House staff member in any administration has a set of previously established ties with groups. While in the White House, these aides maintain and use those associations, they interact with all kinds of group representatives regardless of their formal positions, and they often act as group advocates in the course of White House deliberations (Greenwald 1977, Pika 1985, 5; 212). That is the nature of the Washington political community, independent of whatever liaison structure may exist in the White House. Among the groups I examined, if an organization had any interaction with one part of the White House establishment, chances are that it also had some type of relationship with others in the EOP. For example, the Pearson's-*r* correlation between interacting with the OPL and other staff members in the White House Office is .62. The OMB is somewhat more autonomous, the OPL–OMB correlation being .36.

Despite these overlapping associations, there are some significant variations. Almost 30% of the organizations that had frequent contact with other members of the White House Office staff seldom, if ever, interacted with the OPL. Of those experiencing frequent interaction with the OPL, 12% rarely dealt with others in the White House Office. The distinctions are even more pronounced for the OMB. Of the groups frequently communicating with the OPL, 56% seldom or almost never met with officials in the budget office, while more than half of the groups most actively engaged with the OMB almost never communicated with the public liaison staff. These differences can be traced to the divergent parts of the interest group community being catered to by each office within the EOP and to how the different aspects of interest group liaison comported with their agendas.

Jack Walker developed a typology of membership groups that differentiates among associations based upon their ties to distinct occupational communities and their motivation by an idea or cause (1983, 1991). The typology includes four categories of organizations emanating from different sectors of the political economy: (1) groups whose members engage in the same profession or commercial activity in the profit-making sector of the society; (2) groups whose members are employed mainly by the governmental or private nonprofit sector of the society; (3) "mixed" groups, whose memberships come about equally from the profit and nonprofit sectors; and (4) citizens groups motivated by a cause or idea, whose members need not share any common occupational characteristics. (Trade unions are treated as a separate fifth category.)

There are noticeable differences in the types of interest organizations that maintained relations with various segments of the Reagan White House. The organizations that interacted frequently with the OPL constituted a much broader cross-section of the interest group world than was true of the other components of the EOP, including a fairly even representation of the profit (31%) and nonprofit (28%) sectors. More significantly, citizens groups were particularly prominent among those having the most frequent contact with the OPL, comprising nearly 40% of that category. (They constitute less than a quarter of the whole sample.) The citizens groups themselves, however, were a diverse lot, reflecting the contradictory impulses of liaison as outreach and liaison as legitimization emphasized by the public liaison office. Some were groups committed to conservative causes who enjoyed a cooperative relationship with the White House.[10] Other organizations with OPL contact were the liberal groups enjoying the benefits of institutionalized access. Even with this liberal representation, however, most of the liberal citizen groups reported that they seldom or almost never interacted with the OPL and that when they did, they found the White House to be less than cooperative. In the panel data set, 75% of these groups (organizations in environmental, consumer, and animal rights movements and supporters of social services and a less confrontationist foreign policy) reported that the election of Reagan generally disrupted their relationships with agencies throughout the government. In 1980, 57% had perceived an increase in cooperation as a result of Jimmy Carter's becoming president.

The situation for the OMB is dramatically different. Citizens groups were hardly counted at all among the groups interacting frequently with the budget office (8%), while a significant majority (59%) were trade associations and other organizations representing the profit sector of the society. Perhaps what we see here is a closer approximation of the traditional Republican coalition, made up of organizations representing business interests, large and in possession of considerable resources, conservative and Republican in the conventional sense. Their attention is focused on the brand of economic and regulatory issues that dominated the initial agenda of the Reagan administration, not the social issues pressed by the cause-oriented groups of the New Right.

More significantly, unlike the nascent organizations of the New Right, these groups had close ties to many parts of the government where officials preferred the quiet maneuvering of the backroom to the rollicking politics of open confrontation. From questions about the political activities of groups participating in the study, one can construct an index indicating the extent to which organizations emphasized the importance of *outside* political strategies, such as using the mass media and engaging in public protests, over *inside* political strategies, such as legislative and administrative lobbying. The scale ranges from −1.73 to +2.54, with negative values corresponding to a concentration on inside strategies (Gais, Peterson, and Walker 1984; Gais and Walker

1991; Walker 1991). In general, groups regularly involved with the EOP were insiders (−.56 on the index) while those with little or no White House contact clearly employed outside strategies of influence (+.63). But when one compares the groups with frequent or occasional interaction with the OMB to those that exhibited similar contact with the OPL, the insider quality of the OMB groups really stands out. They rate a −.69 on the scale, devoid of the rabble-rousers who move the mean for the OPL activists up to −.29. It should be noted, however, that the very fact of such extensive and politically based involvement with the OMB by even such insider interest groups is itself testamony to how the budget bureau has changed from an atmosphere of "neutral competence" to one of explicit responsiveness to the president (Berman 1979; Heclo 1975; Moe 1985).

A Multivariate Confirmation

A final test of the hypothesis that President Reagan pursued a strategy of interest group liaison as governing party can be accomplished by employing an ordered multinormal probit analysis to control for the simultaneous presence of the attributes I have examined. The results of the probit analysis, presented in Table 1, confirm the overarching thrust of the interest group liaison strategy pursued by the White House in Ronald Reagan's first term, as well as the variations in the liaison approaches associated with different components of the EOP.[11]

It is strikingly clear from the multivariate analysis that the conceptual elements required of the liaison as governing party approach were well represented in the Reagan White House's relationships with interest organizations. Across all units of the EOP, interactions were more frequent with organizations that demonstrated Republican partisanship (and were decidedly not unions), possessed extensive staff resources, and extensively lobbied Congress. Even the activities of the OPL conform in the aggregate to that expectation. However, an added measure of potential group resources, the existence of a decentralized organization with state or local subunits, proved meaningful only for the OPL and may have little to do with governing party activities.

The governing party thesis could easily have been disconfirmed by this analysis. Republican partisanship without a role for staff resources, lobbying activity, and contrary constituent groups would have signified liaison as outreach. An insignificant coefficient for partisanship combined with statistically significant positive associations for constituent and citizen groups (and perhaps labor unions) would have indicated liaison as legitimization. Insignificant values for partisanship and group types combined with positive coefficients for staff resources and legislative lobbying would conform with liaison as consensus building. The lack of any significant coefficients would suggest the absence of any interest group liaison strategy at all. None of these situations appears in Table 1.

TABLE 1

Probit Coefficient Estimates for Frequency of Group Interactions

GROUP ATTRIBUTES AND TYPES	INTERACTING OFFICE: ANY OFFICE IN THE EOP	OPL	OMB
Group attributes			
Republican partisanship	.481***	.336***	.318***
	(.083)	(.086)	(.085)
Staff resources	.112**	.095**	.158***
	(.038)	(.040)	(.040)
Decentralized organization	.079	.200***	−.087
	(.089)	(.095)	(.095)
Active in congressional lobbying	.359***	.212***	.429***
	(.029)	(.031)	(.035)
Active in congressional elections	.002	.039	−.069**
	(.029)	(.030)	(.030)
Group type			
Profit sector	.046	.316**	.023
	(.108)	(.116)	(.115)
Mixed profit–nonprofit	−.110	−.100	.181
	(.195)	(.222)	(.205)
Citizen group	−.182	.169	−.326**
	(.120)	(.129)	(.129)
Constituent group	.137	.386**	.081
	(.201)	(.205)	(.211)
Labor union	−.649**	−.465	−.562**
	(.286)	(.322)	(.320)
Number of cases	719	716	715
Percent correctly predicted	32.7	36.9	36.8

Note: Values in the table are ordered multinormal probit coefficient estimates. The standard errors are reported in parentheses.
*$p < .10$, two-tailed test.
**$p < .05$, two-tailed test.
***$p < .01$, two-tailed test.

The subtle differences across the units of the EOP that capture elements of the different patterns of interest group liaison, however, are revealed in the multivariate setting. The outreach and institutionally mandated legitimization activities of the public liaison office can be seen in the coefficients for citizen groups and constituent-based organizations. While there is a negative or statistically insignificant relationship between these types of groups and the frequency of interaction with the EOP as a whole and the OMB in particular, the coefficients are positive for the OPL. The involvement of conservative cause-oriented groups in Reagan's electoral coalition and their possible activity in congressional-level campaigns may explain the positive, although not statistically significant, coefficient linking involvement in congressional elections with OPL activity.[12]

The interest group associates of the OMB, on the other hand, fit both the model of liaison as governing party and the definition of consummate inside Washington players. Republican partisanship, staff resources, and lobbying Congress are essential attributes of groups' interactions with the OMB. The obvious politics of electioneering and the cause orientation of citizen groups, however, are negatively related to group involvement with the budget office. The coefficients for the types of groups interacting with the OMB show the importance of occupationally based associations in contrast to the citizen groups. During the first term of the Reagan administration there was no evidence of outreach and legitimization activities being conducted by budget office staff.

DISCUSSION

If the personnel in President Reagan's OMB were engaged in liaison as governing party (as they seemed to have been, in earnest), then it is a "governing party" well matched to the old definitions of the Republican coalition. The public liaison office, on the other hand, had the difficult task of pursuing the governing party strategy while simultaneously accommodating the interests of both those on the Right, historically denied ready access to government, and the liberal organizations whose fundamental tenets were at odds with the goals of the administration. I suspect that the non-OPL staff members located in the White House Office came closest to managing a version of liaison as governing party that reflected the particular coalitional alignment of Reagan's Republican party. It was more freewheeling than the business-dominated groups tied to the OMB but less buffeted by forces on both the Left and the Right that complicated the role of the OPL and undoubtedly diminished its intra-institutional influence.

If the questions about liaison activities had been asked in the 1980 survey, the results would have been similar in some respects and quite different in others. Given the programmatic orientation of the Carter administration, the attributes of liaison as governing party would have been as strong, but with "Democratic" partisan groups and citizen associations gaining the advantage. With the legislatively directed leadership of Anne Wexler as head of the OPL and the lesser presence of long-standing institutional ties of conservative constituent groups to the White House, liaison as governing party would have been rooted in the public liaison office without much distraction created by other forms of interest group liaison. Other members of the White House office would have reflected the outreach and legitimization efforts intentionally separated from the purview of the OPL (Scholzman and Tierney 1986, 326).

If the interest group survey had been administered again in 1990, the results would also have been somewhat different from 1985. By the second half of the Reagan administration, the institutional vitality of the OPL had diminished even further. With the return of fully divided government after the 1986 election, the replacement of pragmatist James Baker as chief of staff by former treasury secretary Donald Regan, the diminution of the administration's legislative agenda begun in 1982 but made complete, and the disclosure of the Iran–Contra affair, White House liaison with interest groups and its mechanisms were transformed. The public liaison staff lost their autonomy as they became an appendage of the White House Communications Office, directed by stridently conservative Patrick Buchanan. The mission of the public liaison operation shifted decisively to an emphasis on liaison as conservative outreach, not one of governing party or even legitimization.

The first years of George Bush's presidency have done little to improve the institutional situation of public liaison, even as the overall priorities of interest group liaison once again changed. The White House no longer has a director of public liaison with senior staff rank; and the OPL staff dropped to 15 people, down from the 56 in the Reagan White House (Kilberg 1991). The OPL director, now a deputy assistant to the president, remains within the fold of the Communications Office. However, Bobbie Kilberg, Bush's first OPL director, had a decidedly more pragmatist cast than her predecessors in the Reagan administration. She expressed the belief that groups representing all views should have access to the White House (Kilberg 1991).

As the political reach of President Bush has widened over that of his predecessor, and he has sought a more even balance between the programmatic and representational aspects of presidential leadership (submitting a few major legislative packages, such as the clean air bill and the drug initiative, while also nurturing ties to the Right and offering conciliation toward traditional liberal groups), interest group liaison functions therefore serve primarily, with some exceptions, the purposes of both consensus (perhaps compromise) building and legitimization (see Duffy 1989). One approach had even the president in separate two-hour sessions meeting directly with business representatives and environmental groups to discuss the Clean Air Act. The other had the OPL organizing, in a typical week, South Lawn functions for 900 educators, 1,600 Hispanics, and 262 German–Americans, as well as eight major policy briefings and five group photo opportunities in the Oval Office (Kilberg 1991).

As changes occur in the larger political context and in presidential needs or objectives, the character of liaison activities and their prosecution adjust accordingly. For now, they seem quite stable. Unless President Bush suddenly develops a more ambitious legislative agenda, especially in issue areas devoid of the potential for consensus, the Republican equivalent of Anne Wexler, steeped in the notion of governing party, will find little to do in the remainder of what some have called Ronald Reagan's "third term." It will take a future more activist Republican president or a Democratic administration to create once again the impetus for change.

Because of the recent significance of liaison as governing party and the continued practice of the other forms of liaison, considerably more is involved here than simply tracing the activities of one White House office among many. As I have demonstrated, liaison between the White House and the group system ranges far beyond the duties of the OPL alone. White House liaison with interest groups writ large has become a central factor in the overall presentation of the president within the legislative process and within the polity as a whole.

Modern interest group liaison also has much to do with how we should interpret the dynamic relationship between interest groups and the party system and between the interrelated group and party systems and the society they together reflect (Peterson 1992). The president, as political party leader and as recently emergent facilitator of group coalition building, serves as a point of convergence for these institutions. As the grand (or at least incessant) speechmaker and increasingly a routine purveyor of group interactions, the president also serves to merge the mass public with the organized public. The interest group liaison capabilities of the White House, supplementing the president's traditional party ties and contemporary command of public attention, add to the instruments available for exercising presidential leadership in the modern age.

Notes

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1. From a personal interview. Many other quotes in the article are from personal interviews conducted by the author as part of a larger study examining presidential–congressional relations (Peterson 1990). Those interviewed include former Presidents Jimmy Carter and Gerald Ford, 63 individuals with experience in the Executive Office of the President in the 1953–86 period, 33 persons with experience on Capital Hill during the same period, and 9 journalists. Interviews were completed with three-quarters of those contacted. All respondents, excluding Presidents Carter and Ford, were promised that the interviews were not for attribution.

2. These characteristics are similar to the factors suggested by Pika as sources of variation in group proximity to the president, including their "degree of organization, experience in national affairs (sophistication), . . . availability of agency or political allies," and the much harder to measure "quality of leadership" (1985, 13).

3. The 1985 survey included questions asking the respondents to state the level of services provided by the federal government that their associations supported in 10 policy areas and the level of federal regulation that they considered appropriate in 9 policy areas. I combined the responses to these questions to create two summary ideological scales, one for the provision of services and one for regulation. Each scale has the categories *very liberal, liberal, moderate, conservative and very conservative*. The liberal and conservative labels may be somewhat misleading, however. A group favoring the present level of services in 1985, for instance, preferred a level of government involvement that was lower, as a result of the changes in budget priorities advanced by Reagan, than a few years earlier. The panel data set created from the 1980 and 1985 surveys shows that of the groups desiring the 1985 status quo in the overall provision of government services, 42% reported wanting less in the way of government services when they completed the 1980 survey at the end of Jimmy Carter's term. Fully 60% of those pleased with the "present level" of services in 1980 desired increased federal activity by 1985. The status quo, or "moderate," position in 1985 may be interpreted more accurately as a "conservative-leaning" position.

4. In the 1985 survey, the groups were asked to rate on a scale of 1 to 5 (*poor* to *good description*) whether their organization "perceives an important difference between the two major political parties on those issues most relevant to its goals." Responses of 3, 4, or 5 were treated as indicating a sensitivity to partisan politics. Those groups were then divided into "Democrats" (*very liberal* and *liberal*) and "Republicans" (*very conservative* and *conservative*) for each kind of federal intervention, with Republicans being defined as groups that favor less provision of federal services (except in the case of defense) and less federal regulatory intervention.

5. For those who provided information about all three resources, the product moment correlation between the number of staff members and amount of revenues is .72; between size of staff and membership it is about .45 (with all values logged).

6. The ordinal categories of each dimension used for generating the tau-B statistics are *frequent*, *occasional*, *seldom*, and *almost never* for interactions with the EOP and *large staff with legislative experience*, *large staff without legislative experience*, *small staff with legislative experience*, and *small staff without legislative experience* for staff resources.

7. The small–large distinction is made by dividing the sample at the point of the median staff size of 10.

8. The role of electoral activity, unfortunately, cannot be given an adequate test with the data from the interest group surveys. The respondent organizations are typically the tax-exempt nonadvocacy wing of multifaceted organizational enterprises. As a result, due to regulations restricting the activities of organizations possessing such tax status, relatively few groups (about a third in the sample) admitted to engaging in electoral activity. Only 6% gave it the highest priority, contrasted with the 47% who assigned that priority to legislative lobbying. The more useful information would be to determine which of the groups have affiliated political action committees and whether such affiliations make them more attractive to the administration.

9. See n. 8.

10. Undoubtedly this set of conservative "groups" is underrepresented in the sample, and would include a considerable number of nonmembership organizations not captured in the surveys. Many conservative groups probably also have access to the public liaison office through various attorneys and consultants, such as Robert Gray, who are the hired hands of representation (Berry 1984, 194).

11. The explanatory variables have the following values: *Republican partisanship* −1 = "Democratic" groups, 0 = neither party, 1 = "Republican" groups, based on the ideology and party questions discussed earlier. *Staff resources* 1 = small staff with no legislative experience, 2 = small staff with legislative experience, 3 = large staff with no legislative experience, 4 = large staff with legislative experience; small staffs have less then 10 full-time equivalents. *Active in congressional lobbying*

and *active in congressional elections* 0 = not engaged 1 = one of the least important activities, 5 = one of the most important activities (2–4 being continuum points). *Profit sector, mixed profit–nonprofit, citizen group,* and *labor union* are dummy variables created from Walker's (1983, 1991) group typology (with nonprofit sector groups excluded). *Constituent group* 1 = originally identified in the 1980 survey as a black, aging, Hispanic, handicapped, or women's group, 0 = all other groups (dummy variable). *Decentralized organization* 1 = has state or local offices, chapters, headquarters, or other similar subunits; 0 = does not have subunits (variable added to the probit analysis).

12. See n. 8.

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CORPORATE ELECTORAL ACTIVITY, CONSTITUTIONAL DISCOURSE, AND CONCEPTIONS OF THE INDIVIDUAL

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Recently, constitutional scholars have begun exploring how far constitutional discourse is governed by competing liberal and republican conceptions of the just or the good society. At the same time, political theorists have placed in doubt how far this debate rests on the metaphysical dichotomy between a radically autonomous and a radically encumbered individual. Scholars, however, have yet to investigate to what extent metaphysical conceptions of the autonomous or the encumbered individual underlie and determine strands of constitutional discourse. I analyze the deep structure of constitutional discourse in one important area—the legitimacy of prohibitions on corporate electoral activity. I show that the constitutional argumentation recurrently mounted against corporate electoral activity rests on a particular version of the radically encumbered individual and that the major constitutional defenses of corporate electoral activity assume a particular version of the radically autonomous individual.

Recently, contemporary political–theoretic debate between liberals and republicans has moved in two notable directions.[1] It has both entered the domain of constitutional scholarship and attenuated its connection with metaphysics.

Rejecting "any sharp division between legal and political . . . discourse" and moving beyond homogeneous liberal portraits of American political thought, constitutional scholars have begun asking "whether . . . 'republican' or 'liberal' conceptions" have actually been more "dominant in U.S. public acts in particular eras" (Smith 1985, 7, 6; see also Smith 1988, 90–91; Shklar 1990 and see also Michelman 1988, 1494–95 and Sunstein 1991a, 3. (More generally, see Ackerman 1984; Hirsch 1986; Horwitz 1987; Mashaw 1988, 1685; Michelman 1986; Powell 1988, 1706; Sherry 1986; Sunstein 1988, 1540). Contemporaneously, political theorists have begun questioning the relevance, for the liberal–republican debate, of the "stark" metaphysical "dichotom[y]" between "a richly socially constituted and embedded self, and an autonomous chooser maximizing preferences" (Rosenblum 1987, 153; see also Kymlicka 1989, 55; Neal and Paris 1990, 422). For many years, republican political thought was assumed to rest on a self "embedded in various social roles and communal relationships," liberalism, on a self "cut loose from all social ties, literally unencumbered, each one the inventor of his own life" (Kymlicka 1989, 9; Walzer 1990, 9). But with the publication of several recent essays by John Rawls (1975, 1985), several commentators have come to argue, along with Rawls himself, that liberalism does not presuppose a metaphysics and in particular, that liberal theory "should not be understood as presupposing a . . . metaphysical conception of the person as identified with the individual's capacity for rational autonomy" (Gutmann 1985, 313; Margaret Moore 1990, 460; see also Bernstein 1987, 543). And for their part, republicans now speak of selves only "partly defined by the communities we inhabit" and of "the capacity of the self through reflection to participate in the constitution of its identity" (Rosenblum 1987, 163; Thigpen and Downing 1987, 645).

As the liberal–republican debate in political theory has lately come within the compass of constitutional law, so the metaphysical conflict between the autonomous and the encumbered self has recently grown detached from the liberal–republican debate in political theory. I do not aim either to account for these twin evolutions or to speculate about connections between them. Rather, I am interested in pursuing a question centering on the third side of the political theory–constitutional law–metaphysics triangle. Does the "stark dichotomy" between an abstract and an embedded self, even if no longer pertinent to the liberal–republican debate among political theorists, nevertheless assume a role in the evolution of American constitutional law? Have different versions of the autonomous and encumbered individual—not simply (as many constitutional scholars are now beginning to suggest) liberal and republican theories concerning the good or just society—played a part in determining the course of recent constitutional discourse? In asking this question, I am pursuing a variation of the scholarly enterprise that analyzes competing constitutional views in terms of the theories they presuppose (Smith 1985, 6). But instead of showing which "versions of liberal or illiberal *political* thought are important strains in American constitutional discourse today" (ibid.; my emphasis), I will point, within constitutional discourse, to particular versions of (what might until recently have been called) liberal or illiberal *metaphysical* thought.

The question how far constitutional argumentation presupposes autonomous or encumbered conceptions of the individual cannot, of course, be answered exhaustively or conclusively.[2] Indeed, as Smith notes, if one is attempting a "qualitative interpretive narrative that shows [certain] structures of thought and argument to be visible in [constitutional] texts," one can do so only piecemeal, by "[focusing] on a few major cases that seem representative or determinative of most judicial actions in a given area, instead of documenting how those structures are visible in all or most of the relevant cases" (1988, 91, 102, 104–6). Accordingly, I am going to examine how competing conceptions of the self—autonomous versus encumbered—have largely governed one important strand of recent constitutional discourse, namely, the debate over the constitutionality of limitations or prohibitions on corporate electoral activity.

DEFINING SOME TERMS OF DISCUSSION

Before examining the crucial role that metaphysical conceptions of the self have played in constitutional discourse over corporate electoral activity, it will be helpful to narrow the meaning of two key terms—*corporate electoral activity* and *constitutional discourse*.

The term *corporate electoral activity* refers to corporate contributions and expenditures in candidate elections and referendum campaigns. It is to be distinguished from other, broader (and less controversial) forms of corporate political activity, such as lobbying legislators, advocacy advertising, and so forth. Debate over these latter varieties of corporate electoral activity has not assumed a significant constitutional dimension.

The term *constitutional discourse* refers to both the opinions of judges *and* the commentary of legal scholars (Smith 1985, 5). Following Smith's suggestion that scholars of constitutional discourse focus on a few "representative or determinative" cases in a given area, the discourse I examine draws centrally (but by no means exclusively) on two major cases: *First National Bank of Boston* v. *Bellotti* (1978) and *Austin* v. *Michigan State Chamber of Commerce* (1990). In *Bellotti*, the Supreme Court struck down a Massachusetts law prohibiting direct corporate expenditures in referendum campaigns. In *Austin*, it upheld a Michigan law prohibiting direct corporate expenditures in candidate elections.[3] Across the vast range of the debate, however, few scholars and judges in fact draw any distinction between the two types of corporate electoral activity; the same arguments are marshaled for—and against—each. Indeed, the patterns of reasoning underlying *Bellotti* and *Austin* are flatly inconsistent; the very arguments that prevailed in the one were vanquished in the other. Judges and scholars thus depict the constitutional issue as one that concerns the essential legitimacy or illegitimacy of corporate electoral activity per se. Viewing it as a question that lends itself to resolution in toto, only one way or the other, each takes a side.[4]

Prior to *Bellotti* and *Austin*, the Court had used various procedural evasions to avoid "squarely addressing" the constitutional merits of the issue in its decisions (Birnbaum 1979, 164).[5] These procedural evasions, of course, did not stop individual judges from generating much substantive dicta in the area or scholars, much substantive commentary. More recently, with *Bellotti* and *Austin*, the Court has traded in procedural evasion for a kind of official equivocation: currently some forms of corporate electoral activity are protected, others not. Right along side the Court's official equivocation, however, individual judges and scholars have remained personally univocal. As a result, two cogent and conflicting schools of thought have emerged—on Court and off—concerning the legitimacy of corporate electoral activity. The unclarity of the Court's official record on the subject only testifies to the intractability of the issue and to the strength that these two schools of thought have shown over time. Together, they comprise the "constitutional discourse" that I shall examine.

I shall show that the deep structure of constitutional discourse over corporate electoral activity resolves itself into a fundamental metaphysical disagreement over the nature of the individual or the self. To see this, it will be necessary first to set forth in a schematic way the four most common arguments that judges and scholars have historically made against corporate electoral activity and to accompany them with the four responses habitually offered by those who defend the corporation's right to electoral involvement.[6] Of course, the disagreement between those who favor, and those who oppose, corporate electoral activity varies in nuance depending on the particular corporate form or instrument under discussion. A corporation may be large or small, widely held or narrowly held, nonprofit or for-profit; and it may engage in electoral activity directly or through a political action committee (PAC). These various possibilities raise considerations that I shall take into account. I shall then show that each of the four constitutional arguments made against corporate electoral activity rests, fundamentally, on a shared assumption that as political actors, individuals are essentially embedded in or constituted by their contextual—social and economic—roles, relationships, interests, and determinants. And I will show that each of the four arguments mounted in defense of corporate electoral activity rests, fundamentally, on a shared assumption that as a political actor, the individual remains essentially autonomous of—or unencumbered by—contextual surroundings.

CONSTITUTIONAL DISCOURSE OVER CORPORATE ELECTORAL ACTIVITY

The First Argument

According to the first recurring judicial and scholarly argument against corporate electoral activity, corporations possess a capacity to advertise, or "hype," their political views to an extent that no private individual or association can hope to match. Electorally active corporations will therefore necessarily exert a disproportionate—indeed, a distorting—amount of influence in the political marketplace, drowning out the voices of those who argue competing positions (*Federal Election Commission* v. *Massachusetts Citizens for Life* 1986, 257; Sunstein 1988, 1576–77; Sunstein 1991a, 30). Wherever corporate electoral advertising is allowed, political ideas will no longer be judged principally by their merit but will, instead, gain acceptability in proportion to the loudness with which they are propounded (Cox 1980, 70; Cox 1982, 418).

Those who make this "distorting influence" criticism, it should be noted, explicitly confine it to large corporations and acknowledge that it applies with much less force to *small* corporations. Small corporations, after all, cannot deploy substantial economic resources in the political marketplace (*Austin* v. *Michigan* 1990, 661). On the other hand, this "distorting influence" criterion has been used to condemn not just direct corporate electoral activity but also electoral activity by corporate PACs. Electoral contributions and expenditures by corporate PACs, of course, draw on funds raised from the voluntary contributions of individual shareholders and employees. Electoral contributions and expenditures by corporations per se, by contrast, draw on corporate treasury funds. Nevertheless, it is frequently urged that the corporate PAC is just as capable as is the corporation itself of bringing large sums of money to bear in the political marketplace, drowning out the merits of competing positions (Wright 1982, 619).

The constitutional defenders of corporate electoral activity habitually respond that in forming their political views, most Americans are fully capable of discounting the economic throw-weight that corporations can place behind their political ideas. Most are mature enough to judge those ideas (along with all others) solely on the merits, unswayed by the wealth-bought hype behind them. As Justice Scalia flatly asserted in his *Austin* dissent "people are not foolish but intelligent, and will separate the wheat from the chaff" (*Austin* v. *Michigan* 1990, 695; 706). Justice Powell, in *Bellotti* urged that "the people in our democracy are entrusted with the responsibility for judging and evaluating the relative merits of conflicting arguments. . . . If there be any danger that the people cannot evaluate the information and arguments advanced [by corporations], it is a danger uncontemplated by the Framers of the First Amendment" (*First National Bank of Boston* v. *Bellotti* 1978, 791–92). Naturally, then, on such a standard any "legislation seeking to minimize the impact of material inequality on politics" must be seen as paternalistic—as "impugn[ing] the capacity of autonomous individuals to fend for themselves in the marketplace . . . of ideas" ("Corporation and the Constitution" 1981, 1854; see also Winter and Bolton 1973, 12). Arguments like Cox's "reflect a basic distrust in the capacity of individual citizens to discount the greater volume of political advertising in reaching their decisions" (Fleishman 1973, 455). Corporate electoral activity, its defenders thus assert, is not likely to addle the political judgment of individual Americans. Nor, they insist, can public opinion be moved in any appreciable way by the sheer firepower of great economic wealth (*Austin* v. *Michigan* 1990, 680, 705).

The Second Argument

According to the second recurrent argument against corporate electoral activity, the government, in granting the corporation special legal privileges such as limited liability, perpetual life, and favored treatment of assets, thereby "provides indirect assistance for any political expression corporations may undertake. This is a violation of the doctrine of [government] neutrality" ("Corporate Political Expression" 1978, 168; see also *Austin* v. *Michigan* 1990, 658–59). Whenever corporations involve themselves in politics, the government, notwithstanding the political diversity of the public it is obligated to represent, is thereby indirectly favoring some political positions (i.e., those of corporations) as opposed to others. It is bestowing society's collective privileges on—and is therefore helping to advance—political views that may well be supported by only a part—indeed, perhaps a very small part—of the public at large (*FEC* v. *Citizens for Life* 1986, 238, 267; *FEC* v. *National Conservative Political Action Committee* 1985, 490). To permit corporate electoral activity is thus to unfairly establish a "state-created amplification system, available only to a relative few" (Patton and Barrett 1981, 511). As Justice White urged in his *Bellotti* dissent, "Massachusetts could permissibly conclude that not to impose limits upon the political activities of corporations would have placed it in a position of departing from neutrality, and indirectly assisting the propagation of corporate views, because of the advantages its laws give to the corporate acquisition of funds to finance such activities" (*First National* v. *Bellotti* 1978, 809–10). The principle of government neutrality, out of a respect for American political pluralism, therefore requires incorporated businesses to stay out of electoral politics.

Of course (as its exponents acknowledge), the principle of government neutrality provides no grounds for objection to electoral activity undertaken by *unincorporated* businesses, which do not enjoy comparable government-sponsored advantages (Michigan State Chamber of Commerce 1989, 95a–96a). On the other hand, the principle has been used to condemn electoral activity by corporate PACs. True, the funds that the corporate PAC expends in political campaigns are not drawn from corporate treasury funds and hence cannot be attributed to any state-granted privileges. Nevertheless, the monies required to support the activity of corporate-PAC fund-raising itself, as well as the PAC's day-to-day operating expenses, are allowed to come from the corporate treasury. For this reason, corporate-PAC electoral activity, like direct corporate electoral activity, has been criticized for intruding the state-advantaged corporate form into the political process, with an attendant violation of government political neutrality (Mayton 1980, 380–81).

In response, those who defend corporate electoral activity have historically pointed out that many individual Americans (and not just corporations) receive assistance from the government, in the form of tax concessions, jobs, welfare, and the like. Just because the government advantages these individuals *economically*, however, we do not conclude that the government is somehow speaking through them *politically*, violating its neutrality and favoring their political views to the detriment of those who receive no government support. "Those individuals who form that type of voluntary association known as a corporation are, to be sure, given special advantages—notably [limited] liability," Justice Scalia argued in *Austin*, but so are other associations and private individuals given all sorts of special advantages that the State need not confer, ranging from tax breaks to contract awards to public employment to outright tax subsidies. It is rudimentary that the State cannot exact as the price of those special advantages the forfeiture of [their] First Amendment rights" (*Austin* v. *Michigan* 1990, 680). The connection between the various types of economic support that the government provides to individuals and associations and the particular political utterances they may issue is an attenuated one. Certainly, that connection is too attenuated for the government to be violating its political neutrality every time it hands out a welfare check or lets a contract to a politically active person or association. Indeed (so this argument would suggest), the government supports a great many such individuals and associations, each of whom may take a different political position. If it is true to say that the government speaks politically whenever it assists someone economically, then it is also the case that it is continually neutralizing any previous violations of neutrality.

It is important to explore this line of argument a little further. Defenders of corporate electoral activity, arguing by way of analogy, point out that as a society, we unproblematically deem a welfare recipient's or a government contractor's political expression to be his own and not the government's (see Shiffrin 1980). In similar fashion, we consider a hospital doctor's political expression to be personal, not corporate (Ashdown 1991, 782). To believe otherwise is to diminish the idea that all individuals possess fully developed, self-contained political identities that begin and end with them and that do not indirectly represent, embody, or implicate and are not determined by, the identities of those who support them economically. In its *Austin* brief, the American Civil Liberties Union urged in defense of corporate electoral activity that "an individual who is wealthy because he made a better mousetrap [cannot] be prohibited from spending [his wealth] on political speech, [even] if he is a Socialist and most rodent-hunters are Republicans" (1990, 13, 23–24). Similarly, a corporation that is wealthy because of state-granted privileges cannot be prohibited from spending its wealth on political speech, even if it supports Republicans and most citizens of the state are Democrats. The corporation and the state, like the mousetrap inventor and his customers, are free-standing political entities, whatever their economic links may be.

As a society, then (so it is urged), we believe it illegitimate to consider individuals' and groups' economic sources of support in assigning them rights and status as political citizens. Whatever their economic underpinnings may be, they are irrelevant to—and hence can place no constraints on—individuals' and groups' identities, privileges, and operations as political entities. And the same applies, it is argued, to corporations. "The benefits granted by modern government are so pervasive that Justice White's argument might justify control of most individual and group expression," the *Harvard Law Review* concluded in its review of the *Bellotti* decision; hence "the argument reaches too far and must be rejected" ("Corporate Political Expression" 1978, 169). For these reasons (so the defenders of corporate electoral activity habitually argue), we should not prohibit corporate electoral activity on the mistaken grounds that such activity represents a violation of government neutrality.

These first two constitutional disagreements surround the question of whether corporate electoral activity intrudes into the political marketplace (1) great private wealth and (2) the political power of government. As such, they are concerned with the effect of corporate electoral activity on the world external to the corporation, the forum in which society conducts its public debates. The final two arguments, by contrast, concern the effect of corporate electoral activity on the world internal to the corporation, that is, on shareholders and how they exercise their political rights.

The Third Argument

Those opposed to corporate electoral activity traditionally urge that whenever corporations take positions in campaign politics, shareholders as a whole are being made to contribute to political positions that only some of them (indeed, perhaps none of them) may support (*First National* v. *Bellotti* 1978, 812). In that they necessarily provide economic assistance, however unintended, for any electoral activity that management chooses to undertake, shareholders are in effect deprived of a measure of control over their own political expression. The regulation of corporate electoral activity, it is argued, then becomes requisite if we are to protect the "substantial interest each individual has in his own political activities" (*United States* v. *Pipefitters Local Union No. 562* 1970, 407). For in finding themselves publicly associated with corporate electoral positions that they personally oppose, dissenting shareholders may well deem their own political voice, their own political support, to have been hijacked: "The group voice, by its very nature, tends to drown out the individual voice, and to identify the dissenter, in the public eye, with the views of the group he opposes" ("Comment" 1962, 789). Critics of corporate electoral activity have thus repeatedly characterized shareholders as "captive speakers" (*Austin* v. *Michigan* 1990, 674–75; "Constitutionality of the Federal Ban" 1974, 153; "Political Speech" 1977, 1037). In a similar vein, Alexander Heard once observed that "membership in any organized group possessed of political interests commits an individual to indirect involvement in the political process, [and the] character of this involvement will be determined by those who dominate the group. There is no escape from politics for the individual" (1960, 210).

In short, with electorally active corporations, managers will continually "impose their political views upon a constituency of diverse convictions" ("Political Contributions" 1979, 416). To legitimize corporate electoral activity is to violate shareholders' rights to free political expression. It is to jeopardize their right to control their own political utterances and involvement and their right to support and be associated only with those candidates and causes they believe in.[7]

This argument, as well, has been modified and extended in a couple of noteworthy ways. To begin with, it does not apply with equal force across the domain of all corporations. As some of those who mount it acknowledge, it may apply with less force (or at least, less reliably) to certain types of closely held or private corporations than to widely held or public corporations (*First National* v. *Bellotti* 1978, 794–5). In the case of such closely held or private corporations, any corporate political expression is arguably more likely to represent the political voice of all shareholders. By the same token, political expression by corporations formed expressly for "ideological" purposes, such as the NAACP, has been deemed even less troublesome (*First National* v. *Bellotti* 1978, 805; *FEC* v. *Citizens for Life* 1986, 238). Yet while private or "ideological" corporations may not fall victim to this particular attack on corporate electoral activity, corporate PACs, despite the voluntary nature of their funding, *have* been attacked for misrepresenting the political positions of their members. Critics point out that the corporate PAC is usually oligarchical in structure. Managed, as it is, by corporate executives to serve corporate interests, the corporate PAC will necessarily take positions that some of its politically diverse contributors would oppose and about which they will very likely remain ignorant. Corporate PACs, it is thus urged, also inevitably misrepresent the individual political voices and collective political diversity of their contributors, just as electorally active corporations do those of their shareholders. As one legal scholar has put it: "The corporation can . . . retain 'control' of the PAC, with the authority to select candidates who will be recipients of PAC funds. . . . The participants are not 'pooling' their resources to support their consensus candidate. They are merely contributing in the blind; the corporation picks the candidate. The participants are not united by common political goals" (Mayton 1980, 391–92; see also Sabato 1984, xiv and Sorauf 1984).

To this argument, the defenders of corporate electoral activity typically respond that just because an individual may temporarily be associated with a corporation, having bought its shares for personal economic purposes, we do not, as a society, therefore habitually read into the shareholder that corporation's ongoing political views. We recognize that it is the nature of a free market economy that "all monies to some extent are diverted from their sources to other uses," but we do not routinely impute those uses to the sources, especially if there are "a number of steps in the diversion" (Ashdown 1991, 782; see also *Austin* v. *Michigan* 1990, 707). We traditionally regard the purchase of shares, even in an electorally active corporation, as a purely economic act, not a political gesture (Ashdown 1991, 783). We know it to be an exercise of economic calculation, not political speech; we understand that "the shareholder's primary motivation in investing is to obtain an economic benefit, not to make, or refrain from making, a political statement" (Prentice 1981, 639). We are thus capable of distinguishing any given individual's politics from those of the economic entities with which the individual might be associated—or, as one proponent of corporate electoral activity has put it, "the shareholder is not meaningfully . . . making a speech whenever the corporation acts politically," since "the corporate voice is not the individual's voice" (Associated Industries of Massachusetts 1978, 23). Indeed, most shareholders invest in a number of corporations, each one of which may take a different political position. If we believed that individuals always expressed themselves politically through their economic activities (so this defense of corporate electoral activity suggests) we would have to regard such individuals as politically incoherent.

It is thus understood that we do not express ourselves politically in everything we do; nor, especially, in every transitory, secondary, pecuniary association we make (Epstein 1969, 258). To believe that we do is to diminish those occasions on which individuals consciously, directly, and with real intent and meaning take political stands. As a society, we are committed to the idea that all citizens have fully developed political identities and voices to which their economic activities and associations are—and should be considered to be—irrelevant and immaterial (King 1962, 869, 874). Electorally active corporations thus do not "drown out" the voices of dissenting shareholders, identifying them with the views of the corporation. Individual shareholders' political voices and expressions are simply too strong and independent for that to be a concern. Their individual political "identities" remain clearly distinct from those of the institutions that surround them. When we are understood to express ourselves politically, it is full-throatedly through our various civic activities and conversations, not unintentionally and indirectly through our nonpolitical activities.

The connection between the shareholder's purely economic act of investment and the corporation's political utterances is thus simply too remote and indirect for us to deem the corporation to have hijacked anything resembling the shareholder's voice. "Any argument that the . . . shareholder is being forced to speak when corporate funds are used for political expression is unsound," one proponent of corporate electoral activity has thus concluded, because the "use of corporate funds for such expression does not represent a personal endorsement of that view by the shareholder" (*International Association of Machinists* v. *Street* 1961, 740). As another has observed, "Various organizations, as well as governments, spend money in a manner opposed by certain members, yet this is not considered as the speech or expression of these individuals" (King 1962, 875). Money is not speech, it is urged, nor is investment endorsement; and when we act as shareholders, we do not act as citizens. We speak, endorse, and live our political lives elsewhere, in another sphere. Corporate electoral activity thus does not hijack shareholder political support and hence does not offend shareholder political diversity.

The Fourth Argument

According to the fourth recurrent argument mounted by the opponents of corporate electoral activity, shareholders typically invest in a corporation on the basis of its profitability. They thus acquire a significant economic "disincentive" for divesting should they disagree with any political stands it happens to take (*Austin* v. *Michigan* 1990, 653). Thus, the widely held electorally active corporation not only is likely to politically misrepresent various shareholders at any given time but will (assuming it is profitable) economically constrain them to remain with the corporation over time. To use Albert O. Hirschman's (1970) serviceable terminology, not only is shareholder "voice" co-opted by electorally active corporations, but shareholder ability to "exit" is curtailed. In a world of electorally active corporations, it is argued, shareholders' economic interests will become a drag on their ability to act freely and frictionlessly (i.e., on the basis of principle and conviction) in the political sphere. "Many individuals faced with the loss of a favorable investment," one legal scholar has written, "will choose to sacrifice their political ideals. Some will maintain their investments simply out of an inertia that may compromise even their most firmly-held convictions" (Nicholson 1980, 1005; see also Brudney 1981, 257–64). Or, as Justice White put it in his *Bellotti* dissent (*First National* v. *Bellotti* 1978, 818), "The State has an interest . . . in enabling individuals to exercise freedom of conscience without penalty."

This final argument has, like the others, been adapted to take into account the different forms and instrumentalities of electorally active corporations. On the one hand, one often finds it modified by the acknowledgement that electoral activity by *nonprofit* corporations—in which the politically dissenting shareholder who chooses to divest faces little or no economic penalty—may be acceptable even if electoral activity by for-profit corporations is not (see, generally, *FEC* v. *Citizens for Life* 1986). Corporate PACs, on the other hand, are frequently criticized for using the threat of economic "penalties" to extract and retain the political support of their "members"—that is, the employees and shareholders who contribute to them. Corporate PACs, in other words, are criticized not only for being politically oligarchical in their structure but for being economically coercive in their practices, as well (see, e.g., Sabato 1984, xiv, 64–66). The corporate PAC is said to confront both employees *and* shareholders with the threat of suffering financially in a variety of indirect ways if they do not surmount their contrary political convictions and contribute: "Employees do not make [PAC] contributions voluntarily," it is said, "but out of 'fear of disfavor' or 'hope of gaining in employer favor' " (Handler and Mulkern 1982, 58). And although "shareholder volition in making contributions is not as easily coerced as employee volition," PACs nevertheless pressure shareholders to contribute by appealing to—even manipulating—their concerns about their economic interests (Brudney 1981, 272). "When corporations . . . are permitted to solicit contributions from their . . . members" through the PAC system, it is argued, "the danger of coercion arises" (Vandegrift 1980, 470).

Defenders of corporate electoral activity have traditionally responded that as political actors, individuals are quite capable of transcending and even flouting their own economic interests (American Civil Liberties Union 1990, 20). They are capable, in other words, of acting freely and frictionlessly on the basis of political conviction even when faced with an economic disincentive for doing so. Thus, even supposing that corporate electoral activity politically misrepresents dissenting shareholders, those shareholders can easily leave: "The shareholder invests in a corporation of his own volition and is free to withdraw his investment at any time and for any reason"; the corporation's members "voluntarily join it and can voluntarily get out of it" (*First National* v. *Bellotti* 1978, 794; *International Association of Machinists* v. *Street* 1961, 788). These comments affirm a belief in the shareholder's capacity to act—politically—completely on personal volition notwithstanding any countervailing economic interests. According to this belief, it is simply false to say that an individual's shareholding in an electorally active corporation rises to the level of meaningful coercion (Associated Industries of Massachusetts 1977, 23). Selling one's stock for political reasons, Justice Scalia argued in his *Austin* dissent, thus "does not ordinarily involve the severe psychic trauma or economic disaster that [the Court's] opinion suggests" (*Austin* v. *Michigan* 1990, 687). The ability to act independently and voluntarily in the political sphere remains as undiminished for the shareholder as for any other citizen (Prentice 1981, 639). Allowing corporate electoral activity does not mean that shareholders' political convictions will be compromised by their economic interests.[8]

EMPIRICAL SOCIAL SCIENCE AND CONSTITUTIONAL DISCOURSE

Taken together in all their manifold forms, the four disagreements discussed essentially comprise constitutional discourse over corporate electoral activity. Before showing that a particular disagreement over the nature of the self lies at the root of that discourse, however, it is important to address the relationship between constitutional discourse over corporate electoral activity and recent empirical work in the area. One might want to know, for example, whether empirical scholarship can help resolve (or explain) some of the four constitutional disagreements over corporate electoral activity. It is also worth asking whether the study of constitutional discourse over corporate electoral activity might fruitfully reorient (or complement) various strands of empirical scholarship. Here, two bodies of empirical work, in particular, are pertinent: empirical studies of corporate electoral activity and empirical studies of the beliefs and motivations of those, such as judges, who dominate constitutional discourse.

At best, empirical studies of corporate electoral activity currently bear an attenuated relationship to constitutional discourse in the area. In principle, at least some—and perhaps all—of the four constitutional subdebates over corporate electoral activity are amenable to some kind of empirical resolution. As a practical matter, however, "the new attention to the corporation by political scientists has not yet produced a rich stock of empirical knowledge about what business firms do in politics" (Eismeier and Pollock 1988, 2). Empirical scholars disagree over the extent to which corporate electoral activity in actuality "distorts" the workings of the political marketplace, for example, just as they contest the extent to which corporate PACs effectively "coerce" potential members into contributing (see, *First National* v. *Bellotti* 1978, 780; Handler and Mulkern 1982, 57–58, 97–98; Shockley 1985). Even if some of these questions were to be resolved empirically, it is by no means certain that the Court would pay much heed. As Sorauf observes, "The Court [has] never developed ways of relating its doctrine [in the area of corporate electoral activity] to realistic burdens of proof. It set—and allowed trial courts to set—standards of empirical proof for the governmental interests behind legislation that were beyond reason and beyond the standards of informed scholarship" (1986, 119). More generally, "those who study and discuss constitutional law almost never bother with such empirical questions," even though "what they say almost always assumes answers to them" (Sunstein 1991b, 36; see also Segal 1986, 941, 953). To explain and account for judicial and constitutional-scholarly ideas, then, we need to look elsewhere—not in the correspondence of those ideas to the state of empirical knowledge in any given field but in their coherence with deeper ideological preconceptions.

None of this means, however, that there cannot in principle be a fruitful complementarity between empirical scholarship and constitutional discourse in the area of corporate electoral activity; for the deeper, more determinant ideological preconceptions undergirding constitutional discourse may, themselves, be amenable to empirical investigation. The interpretive analysis of constitutional discourse over corporate electoral activity—in particular the exposure of its deepest speculative assumptions—might, thus, usefully redirect some empirical energies. In reconstructing constitutional argumentation, scholars of discourse lay bare the key (and often unexpected) empirical premises assumed by judges and legal scholars. In so doing, they signal to social scientists a new set of questions that must be empirically addressed, if we are ever to move a particular strand of constitutional discourse forward (or at least evaluate it fully). In this case, we might learn that in addition to addressing the question of what "business firms do in politics" per se, empirical scholars might devise ways of validating or refuting less readily apparent—but more fundamentally determinative—metaphysical preconceptions about the nature of the individual-in-society.

There is also a complementarity of sorts between the interpretive study of the deep structure of constitutional discourse, and the social-scientific study of the core beliefs or motivations of individual judges (Segal and Cover 1989, 559; Tate 1981, 365). As Smith observes, "A wealth of . . . internal psychological factors and personal interests . . . influences all political conduct, including the judiciary's"; yet "the behavior of political actors . . . is influenced in part by the nature . . . of the ideas they possess, and the basic ideas of a given period and group often have a discernible structure, which may be articulated in revealing fashion by political writers of the day" (1985, 6–7). This statement is particularly true when the political actors in question are constitutional jurists and legal scholars; for the "very thing that makes constitutional discourse distinctive, its unusually self-conscious effort to connect decisions on immediate conflicts with . . . first principles of the political order, means that if the approach can be usefully applied anywhere, it should be of value here" (ibid.; see also Heymann 1990, 89; Mark Moore 1990, 79). Smith argues that "qualitative studies of the patterns of reasoning characteristic of various strains of legal discourse" can constitute independent "investigations into one dimension of political conduct—a dimension that needs to be assessed like any other if we are to build up a comprehensive empirical portrait of political life" (1988, 90).

Smith's comment suggests that interpretive studies of the deep structure of constitutional discourse and social-scientific studies of the "core belief systems" of individual judges can be viewed as mutually complementary empirical endeavors. In analyzing the deep structure of constitutional discourse over corporate electoral activity, for example, the empirical issue at hand is not whether, as a matter of "internal psychology," individual judges and commentators actually hold to the conceptions of self that their arguments assume. (That is an important, but a separate, issue.) Instead, the empirical issue is whether, at the rhetorical level, judges and commentators publicly justify their arguments by resting them on such conceptions. If so, then to the extent that one or the other of these conceptions is vulnerable to challenge, so is the edifice of argumentation (whether for or against corporate electoral activity) that it supports.

In short, the extent to which constitutional ideas gain acceptability cannot always be explained either by their relative empirical or rational validity or by examining their appeal to judges' particular interests and psychological makeups. Often, their careers can be accounted for only by exploring how far they cohere with deep "ideological structures" (preconceptions and assumptions) concerning aspects of political life (Smith 1988, 104). On the other hand, an interpretive study of those preconceptions and assumptions can usefully complement the social-scientific study of judges' interests and motivations. It can also usefully expand the social-scientific study of the policy questions that come before the courts—in this case, to include the validation of different "empirical assumptions" concerning the individual-in-society.

THE AUTONOMOUS INDIVIDUAL AND THE ENCUMBERED INDIVIDUAL

There is, at bottom, a single thread—a single ideological underlay—connecting the four recurring judicial–scholarly attacks on corporate electoral activity (the electoral activity of large, incorporated, widely held, for-profit businesses), just as there is another such thread connecting the four basic defenses. At the most basic level, constitutional discourse over corporate electoral activity bespeaks something beyond a set of broad disagreements over the nature of the institutions in question (corporations, in particular, but also the political marketplace, government, political associations, and the economic marketplace). Instead, it takes the form of a very defined and persistent disagreement over the conceptual nature of the individual. More exactly, constitutional discourse over corporate electoral activity resolves itself into a disagreement over the conceptual nature of the individual as revealed in its interaction with these various social institutions.

To see this, consider first the conception of the individual underlying the arguments mounted by those who would defend corporate electoral activity. Time and again, judicial and scholarly defenders of corporate electoral activity have depicted an individual who develops political views largely—and admirably—independently of economic considerations. The individual's political opinions cannot be swayed by the economic throw-weight that others might bring to bear behind theirs. Rather, contrary to the fears of Archibald Cox, the individual determines personal politics on the merits. Nor are the individual's political positions hostage to personal economic interests. Instead, contrary to the fears of Justice White, the individual bases political views on conviction and acts accordingly. Large corporations, thus, cannot control the individual's political views with their wealth; nor, as a shareholder, will the individual who opposes its politics have difficulty disassociating from a profitable corporation. When acting politically, the individual is neither so mentally "foolish" as to be controlled by corporate economic power nor subject to "psychic trauma" due to personal shareholder economic interests but "autonomous" of both (*Austin* v. *Michigan* 1990, 687, 695; "Corporation and the Constitution" 1981, 1854). Merit and conviction thus hold sway for the individual; as a political actor, the individual is in no way encumbered by the economic power of others or by particular personal economic interests.

Additionally, those who favor corporate electoral activity have depicted an individual with a developed political identity, one separate and distinct from anything that might be implied about it by any institutional ties or associations. The government's economic support does not mean that the individual has therefore become a mere cipher through which the government speaks. The individual who supports a corporation economically does not thereby have political voice and identity co-opted by it; the individual cannot be construed as either identifying with, or espousing, its views politically.

Those in favor of corporate electoral activity thus posit that Americans, as conceptual individuals or selves, are autonomous and self-constituting political entities, capable of resisting others' economic clout and rising above their own personal economic imperatives. And they posit that Americans, as individuals, possess autonomous and self-contained political identities capable of being conceptually distinguished from their surrounding institutional ties and affiliations. There is an ideological commonality to these beliefs about the individual: together, they begin to approach the conception of the starkly autonomous or abstract self that (at least in some respects and until recently) had been associated with the liberal position in contemporary liberal–republican (liberal–communitarian) political theory debate. It is a self whose political life transcends and remains essentially distinct from its socioeconomic context. (By *socioeconomic context* I mean what would otherwise be a set of constitutive associations and roles and determinative powers and interests.) It is "an individual abstracted from context," a self "identified with a transcendent, autonomous capacity," "standing always at a certain distance from the interests it has," "literally unencumbered, the inventor of his own life" (Hinchman 1990, 759; Margaret Moore 1990, 461: Sandel 1982, 62; Walzer 1990, 9). Whatever its links to defenses of liberal political theory may be, the autonomous, abstract, or transcendent individual is, in its various aspects, *the* central character in the defense of corporate electoral activity.

Those opposed to corporate electoral activity, by contrast, have rested their four main arguments on what, taken as a whole, amounts to a very different view of the individual's defining virtues and features. The individual's political views, they fear, may often be determined more by the firepower of wealth than by the merits; the individual's political actions, more by pecuniary interest than by conviction. And the individual's political identity, they believe, is essentially implicated in, and remains indistinguishable from, institutional associations and roles—in particular, socioeconomic links and activities, whether those of government beneficiary or corporate shareholder. Such a self, in its embeddedness and encumberedness per se, is reminiscent of the more situated metaphysical conception of the individual that, in gross terms, had been associated rather more with the republican position in contemporary liberal–republican political theory debate. It is—perhaps more accurately—simply an "illiberal" or even an "antiindividualistic" conception as George Kateb uses the term (1989, 183–84; Smith 1985, 6).[9] In the words of the political theorists who have examined it, such an individual is variously conceived as "never completely abstracted from the institutions of . . . particular communities," "embedded in . . . social roles and communal relationships," not distinguishable from the interests it has, and "engulfed by social- . . . determinants" (Gill 1986, 126; Kymlicka 1989, 9; Sandel 1982, 62; Thigpen and Downing 1987, 646).[10]

CONCLUSION

Recently, constitutional scholars and political theorists have shown an increasing interest in ascertaining the presence of various ideological structures within constitutional–legal texts, both cases and commentary. I have offered a qualitative narrative that shows that certain structures of thought concerning the conceptual nature of the individual have fundamentally shaped constitutional discourse in the area of corporate electoral activity; for apart from the corporation itself, the "individual" is the only other pervasive conceptual presence in constitutional discourse over corporate electoral activity. When cast at the level of the corporation, however, the four main issues raised in discourse on corporate electoral activity show little relationship one to another: Does corporate electoral activity distort the political marketplace? violate government neutrality? hijack shareholder political expression? economically coerce shareholders? By contrast, the more basic preconceptions of the individual as they animate each side of the four subdebates can be seen to form parts of a coherent whole: an autonomous or abstract individual on the part of those who defend corporate electoral activity, an encumbered or embedded individual on the part of those opposed.

Indeed, it is important to note in passing that not just the concept but also the word *individual* dominates the discourse I have discussed. Defenders of corporate electoral activity variously assert, as fundamental axioms, that "autonomous individuals can fend for themselves in the marketplace of ideas," that "the corporate voice is not the individual's voice," that "private individuals [who are given] special advantages" by the state do not thereby become its instruments, and so on. Opponents of corporate electoral activity, by contrast, rest their case on such assertions as that "membership in any organized group possessed of political interests commits an individual to indirect involvement in the political process," "individuals faced with the loss of a favorable investment will choose to sacrifice their political ideals," and the like. At its fundaments, the disagreement between constitutional proponents and opponents of corporate electoral activity is, quite explicitly, a debate over the conceptual nature of the individual-in-society.

Both sides in constitutional discourse over corporate electoral activity thus advance as root ideological assumptions and assertions starkly competing conceptions of the individual-in-society; in neither case does anything further lie beyond or beneath. The links between these two metaphysical conceptions of the individual and the reigning political theory debate between liberals and republicans are complex and obscure, if not entirely eroded. Nevertheless, they do govern at least one important strand of constitutional discourse. As such, they belong to the "patterns of thought" that political theorists and constitutional scholars should now be searching for in the deep structure of constitutional debate.

Of course, if we are to draw any general conclusions about judges' use of metaphysical conceptions of the individual, we must begin by analyzing Court decisions in a number of areas in terms of their reliance on various versions of the autonomous and encumbered selves. The fact that these preconceptions are so fundamentally determinant to a subject as seemingly unrelated to them as corporate electoral activity, coupled with their status as axial ideological constructs, suggests that there is no reason ab initio to delimit the range of constitutional questions where one might expect to find them playing an important role. I conclude that scholars might fruitfully begin exploring the extent to which metaphysical conceptions of the individual, not just various political theory conceptions of the good or just polity, govern judicial doctrine in other areas of constitutional discourse.

Notes

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1. Whatever distinctions may exist between the terms *republican* and *communitarian* (or, for that matter, between republicans of past eras and contemporary republicans) is not pertinent for my purposes. To the extent that the debate between liberals and Republicans is material to what follows, it is the *contemporary* debate only—in which republicans–communitarians generally seem content to identify themselves with either term.

2. Following recent usage in the liberal–republican debate, I will use the terms *individual* and *self* interchangeably, while recognizing that in other contexts the two terms may have different meanings (see, e.g., Rorty 1976, 315).

3. By "direct" corporate electoral expenditures and contributions, I mean expenditures and contributions funded by the corporate treasury, as distinct from those undertaken by the corporate political action committee, itself funded by contributions from employees and shareholders. While federal law prohibits direct corporate contributions and expenditures, it permits limited political action committee contributions and unlimited expenditures. The states, by contrast, are still largely divided in their approach to the legitimacy of direct corporate electoral activity. For example, 18 states currently prohibit direct corporate contributions in candidate elections, 14 place no limits on such contributions, and 18 fall somewhere in between (see Boggs and Boyce 1990, no. 3).

4. There is one distinction between candidate elections and referendum campaigns that allows for some coherence in the Court record. The Court has held that the state has a compelling interest in preventing political corruption and has observed that while candidates can return a favor, causes cannot. A concern for corruption, however, is both a relatively confined and a relatively closed issue in the constitutional–legal debate over corporate electoral activity. It is confined because (as just noted) it has been held to apply only to some types of corporate electoral activity (i.e., corporate involvement in candidate elections; see *First National* v. *Bellotti* 1978, 790) and not others. It is closed because both opponents *and* defenders of corporate electoral activity agree that it is a concern within that confined area and, instead, fight their battles on the other issues discussed in the text (see *Austin* v. *Michigan* 1990, 702). Being both a confined and a closed issue, the concern over corruption is not part of the ongoing mainstream of constitutional debate over corporate electoral activity. The remaining major issues are both universal (in the sense that they raise the same considerations about all types of corporate electoral activity) and open (i.e., no relative agreement has been reached on them within constitutional discourse). Consequently, unlike the issue of corruption, these issues divide participants cleanly into defenders and opponents of corporate electoral activity per se (see, the discussion in Sorauf 1986, 106, 116).

5. Until *Bellotti*, the broad question of "whether business corporations have a constitutionally protected liberty to engage in political activities" of any sort had "never been squarely addressed by [the] Court" (*First National* v. *Bellotti* 1978, 822). Until *Austin*, the Court had never passed on laws prohibiting direct corporate involvement in candidate elections, at either the federal or state level.

6. I do not mean to suggest that apart from the four disagreements I will be examining, there has been nothing else said on the subject of corporate electoral activity. I would suggest, however, that these four disagreements are the only ones that recur in constitutional discourse (often in more detailed and complex form than in my summaries) and that raise the most fundamental constitutional questions—questions concerning the nature of neutrality, coercion, unfettered political expression, and unclouded political judgment.

7. In *Austin*, the Court at points conflated this third argument against corporate electoral activity—that electorally active corporations co-opt and hence misrepresent the political support of shareholders—with the first argument noted in the text, namely, that corporate electoral activity distorts the political marketplace. To regulate corporate electoral activity, Justice Marshall opined (*Austin* v. *Michigan* 1990, 660), is to regulate the "corrosive and distorting effects of immense aggregations of wealth . . . that have little or no correlation to the public's support for the corporation's political ideas" (see also Ashdown 1991, 783; *Austin* v. *Michigan Chamber of Commerce*" 1991, 1071–72, 1077).

8. Epstein (1984, 476, 478) makes this argument with respect to corporate PAC electoral activity. With respect to the capacity of the corporate PAC to coerce employees, Handler and Mulkern report that "corporate executives who are solicited [by PACs] do not seem to behave like organization men, and they show resistance to the pressures to give that some insist must be overwhelming" (1982, 97).

9. In discussing the dichotomy between a radically transcendent and a radically situated subject, Kateb writes "We can continue to call our matter the debate between liberalism and communitarianism, but it could more precisely be called the debate between individualism and anti-individualism" (1989, 183–84).

10. Some republicans might argue that while they may well have advanced a self more or less encumbered and embedded, they had in mind its being encumbered by, and embedded in, a more communal and less institutional set of social arrangements than those assumed by opponents of corporate electoral activity. But as Nancy L. Rosenblum points out, the examples of community cited by republicans are "radically dissimilar. . . . [They] range from traditional positive structures like ethnic and kinship groups to economic, ideological, and cultural associations [including] corporate bodies" (1987, 156–57; see also the discussion in Sullivan 1988, 1714–21). The version of the encumbered self underlying constitutional argumentation against corporate electoral activity is, then (naturally enough, given the subject of the debate), one more situated in institutional ties (to corporations and government) than rooted in family or ethnic groups. Other republicans might argue that while they may have advanced a conception of the self encumbered by various institutional attachments, they have shied away from a version of the individual as determined or constituted by particular economic interests. Indeed, some republicans might insist that such an individual is a liberal-pluralistic, not a republican, conception. Here, again, it is important to bear in mind the wide-umbrella nature of the term *republican* (Sunstein 1988, 1547). To begin with, some republicans do, in fact, speak of a self encumbered by "the interests it has", or of an individual "engulfed in social determinants." More broadly, certain strands of republicanism clearly presuppose citizens who are not able to surmount their own private interests, just as prominent strands of liberalism (Kant, Rawls) posit individuals who *are* able to transcend their own private interests (Burtt 1990, 25–26, 29–32; Sunstein 1988, 1549, n. 146; ibid., 1558; ibid., 1567, n. 160). Of course, other strands of liberalism and republicanism do just the reverse (Burtt 1990, 27–29, 32–35; Sunstein 1988, 1548–50, 1573). In short, Sunstein, Burtt, and others paint rich pictures of the liberal and republican intellectual traditions. They show us that we can associate with various strands of liberalism both an individual who transcends, and an individual who is governed by, personal interests. Equally, they show us that we can associate with various strands of republicanism both an individual who pursues the common good and one encumbered by personal economic interests. All the more reason, then, why the self underlying opposition to corporate electoral activity is best described as starkly "encumbered" or "embedded," not "republican" or "communitarian." The terms *encumbered self* and *autonomous self* both carry a large number of specific connotations, and only one particular version of each animates the opposing sides in constitutional discourse over corporate electoral activity.

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WAR AND THE FATE OF REGIMES: A COMPARATIVE ANALYSIS

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Governments are likely to be held accountable for the success or failure of their foreign policies. Consequently, we claim that international wars can, under specified conditions, have domestically instigated consequences for violent regime change in the political systems of the participants. Drawing upon all international war participation between 1816 and 1975, we seek to answer the question, Do wars lead to violent changes of regime and if so, under what conditions? Three hypotheses set out the expected associations of a nation's initiator or target role in a war, the war outcome, and the costs of the war with domestically instigated violent changes of regime. Direct relationships are found for all three and hold even against possible threats to their validity and robustness. The results suggest that domestic politics play a larger role in national security policy than is generally believed by realist or neorealist theorists.

Today those of us living in democratic states take for granted the right to hold governments accountable that fail to advance and protect the well-being of their citizens. The acknowledgment of such a right is not universal, nor is it of such long standing. The right to punish a regime by removing it from power—with force if necessary—was hotly debated among social contract theorists throughout the seventeenth and eighteenth centuries. It remains a contentious issue in China, much of Africa and the Middle East, and elsewhere around the globe even today. Beyond the philosophical question of the "natural right" to punish regimes for failed policies is the empirical issue of accountability. Whatever position one may take on the philosophical issue, it is important for leaders and researchers alike to know whether regimes are held responsible for failed national security policies.

If, as a matter of fact, regimes (encompassing leaders and their policies) are held responsible for jeopardizing the well-being of citizens, then we should expect such accountability to dampen any enthusiasm for risky political adventures. In particular, if the fate of a regime turns on how its national security policies affect its citizens, then those policies must be shaped and constrained by expectations about the domestic political consequences if they fail. According to this perspective, every foreign policy maker must look over his or her shoulder and contemplate the prospective domestic political risks associated with each foreign policy action (Lamborn 1991).

We propose to investigate whether the fate of regimes has been dependent on domestic responses to war experiences over the past century-and-a-half. War-related actions are a good starting point, because war is the costliest policy in which a regime can engage. As such, it is the governmental action most likely to jeopardize citizen welfare. Because war has the potential to be extremely costly, experiences in war are particularly salient for an investigation of the political accountability of regimes. Our principal concern, then, is with the relationship between a government's war experience and the likelihood of a forceful removal of the regime by its domestic opponents.[1]

If a relationship between domestic responses to war performance and regime stability is validated empirically, then the realist or neorealist emphasis on external, structural features of the international system probably understates the importance of domestic affairs in shaping national security policy. Regime leaders, according to a perspective that takes accountability seriously, must consider how their actions will play at home even if an adventurous foreign policy ultimately is expected to prove successful militarily. The quest for enhanced international power and security would be expected also to accommodate the desire to satisfy domestic constituencies that can threaten a regime's survival at home (Bueno de Mesquita and Lalman 1992).

Does international war involvement increase the probability that a nation's political regime will be removed from power through violent means? Historians, sociologists, and political scientists have speculated that it does; but broad, systematic data on the relationship are lacking. No research directly addresses the possible linkage of war to revolutionary domestic upheaval in an explicit manner using a rigorous research design. We attempt to correct this lacuna and in doing so to ascertain how war experiences influence the stability of regimes.

The lack of research on this important possible consequence of war is puzzling. Social scientists seem generally agreed that wars are likely to produce an increase in the probability of a violent regime change, but they differ with regard to the factors believed to explain such changes. Some argue that the resource extraction necessary to fight a war will produce instability (Tilly 1975, 1990). Others argue that rebellion will follow war participation when a regime is unable to extract the resources necessary to meet the demands placed upon it by the population. This leads to a consequent decrease in legitimacy and an increase in dissatisfaction with the product of the state (DeNardo 1985; Lamborn 1991; Organski and Kugler 1980). Still others argue that wars bring about a redistribution of power among interest groups and coalitions "and thus prime society for conflict over values and resources, including revolutionary conflict" (Starr 1991, 26; see also Goldstone 1980).

Despite these assertions, prior empirical research provides little evidence relating war experiences to domestic political upheaval and revolution. To be sure, there is research on the effects of international conflict on all levels of internal political conflict; but the results of this research are either mixed (Wilkenfeld and Zinnes 1973), based upon only casual inspection of the data (Sorokin 1957), or limited to a single society (Rasler and Thompson 1989; Stohl 1980).

In reviewing a number of these studies, Stohl begins with the observation that although it is widely thought that war leads to domestic political conflict, "only a few of the studies actually confront this problem with systematic data" (1980, 316). Stohl's further evaluation of these few studies points out their mixed findings and limitations of research design and interpretation. He concludes that a considerable amount of work needs to be done on the possible effects of international conflict on political instability.^{2}

Since Stohl's review there has been relatively little new empirical research on war and violent regime change. Two studies published in the same year as Stohl's essay but probably not available to him offer examinations of war participation and regime change; but their analyses are brief and limited in range and do not distinguish among various forms of regime change (Siverson 1980; Stein and Russett 1980). Rasler and Thompson (1989) trace out the effects of war involvement on domestic political conflict; but their analysis, while theoretically rich and methodologically sophisticated, is limited to the United States in the twentieth century. As part of his analysis of the relationship between national economic decline and dispute escalation, Russett examines the effects of international conflict participation on rebellion, which he measures by a combination of the numbers of armed attacks on political authorities and the resulting deaths; but no direct relationship is found (1990, 136).

Still, the idea that there is a link between international war and internal political change persists—and perhaps for good reason. Tilly, for instance, asserts, "All of Europe's great revolutions, and many of its lesser ones, began with the strains imposed by war" (1990, 186). Tilly recounts the effects of wars in producing the English Revolution of 1688 and the French Revolution and concludes with the following description of the Russian Revolution: "Russian losses in World War I discredited tsarist rule, encouraged military defections, and made the state's vulnerability patent" (ibid.). Tilly's assertion is accurate; but it addresses only major revolutions, the largest, most visible part of violent regime change. Our interest is in probing the overall linkage between war participation and violent domestic political changes of all kinds—not just the relatively few highly visible cases. A few cases, no matter how widely recognized as historically important, are not the basis for a sustainable generalization (Riker 1957).

There is, consequently, much we do not know about the relationship that might exist between war participation and rebellion or revolution. For example, at what rate do the states that enter wars experience violent changes of regime? Does it make a difference in that rate if the state does well in its performance against its opponents (i.e., wins, rather than loses)? Does it make a difference whether the state is an initiator of the war or the target of some other state's policies? Do differences in the cost of war have a consequence for the likelihood of violent regime changes?

Answers to questions such as these are critically important if we are to understand not only the aftermath of war but *also the judgments and expectations of decision makers that promote its inception*. War is not only about gains in national power or security within the context of the international system. We believe that the causes and consequences of wars extend well beyond the systemic, structural impediments that are the focus of realist and neorealist theories of international affairs (Morgenthau 1973; Waltz 1979). War is also a powerful force shaped by, and giving shape to, domestic political affairs and for making *or breaking* the political fortune of national governments. As such, an understanding of the conditions under which war enhances or diminishes the fate of a regime is important both from the perspective of an interest in reducing the danger of violent conflicts and from the perspective of understanding linkages between domestic politics and foreign affairs.

INTERNATIONAL CONFLICT AND VIOLENT REGIME CHANGE

The fundamental question we wish to explore is whether states experience significant violent, internally motivated political changes following participation in international war and, if so, under what war-related conditions. Despite the dearth of broad-based empirical research, there are reasons to believe that a close connection exists between war and the fate of regimes. The ideas supporting this relationship (as mentioned) relate war involvement to regimes' accountability to the citizens they serve. Our consideration of this question takes place in several stages. First, we focus on the outcome of the war and the role of the participants in shaping this outcome. Then we move to an examination of the effects of war costs in influencing violent regime changes. We conclude with an investigation of the relationship between prior and current state experiences with violent regime change to ascertain whether the effects we observe are spurious or robust.

At the outset, we reason that a failed policy in the domain of international war will render a regime vulnerable to removal. A failed conflict policy can arise in several ways. We view all defeats in war as instances of failed policies. After all, war is extremely costly in lost life, property, and opportunities foregone. Consider that defeat in war almost always alters the loser's freedom of action, reducing the nation's autonomy over its own foreign policy or depriving the vanquished state of sovereignty over some portion of its citizens, territory, or national product (Morrow 1987). Evidence of such losses is likely to be more dramatic and more apparent to the domestic population than is evidence of the failure of economic policies, an area known to influence the survivability of regimes. Moreover, in nations in which an electoral system is not present, or is nonfunctional, war losses are abundantly obvious to members of the elite, who themselves may have both the opportunity and motive for replacing leaders.

Although fighting a war is sometimes warranted by beliefs about the prospects of success or even for purposes of building a reputation for toughness or steadfastness, still mounting *anticipated* costs heighten the impetus a regime faces to settle without fighting (Bueno de Mesquita and Lalman 1992; Wittman 1979). Fighting a losing war reflects upon a government's failure to anticipate adequately the costs being imposed on the nation. Such a failure is, we believe, likely to be punished; and this punishment is expected to increase with the costs of the war. This should be true whether the regime's leaders initiated the fighting or were the target of attack. A losing initiator regime is likely to be held accountable for its aggressiveness. A defeated victim who survives the wrath of the victor still confronts the prospect of punishment by its own citizenry for failing to cut losses and settle up earlier.

Victory in war is not as straightforward as defeat. An initiator who wins has generally gained some benefits for the state that apparently were not obtainable through other means, although the gains often come at a high price. Such an accomplishment is likely to prompt indifference or reward; it is unlikely to provoke punishment of the regime. But a victorious target faces a rather different circumstance. When the target of attack emerges as the winner, there is clear ex post evidence that the government's ex ante policy was a failure. Since an initiator is unlikely to attack unless it expects to do better by fighting than by not fighting, it is likely (though not inevitable) that the leaders of a defeated initiator state misjudged the nation's prospects. Such misjudgments are, to some extent, the responsibility of the loser—hence our hypothesis that defeat heightens the likelihood of punishment. But such misjudgments may also reflect a failure by the target to communicate adequately about its motivation and/or capabilities. Such failure to communicate must at least partially offset any domestic political benefits to be had as a result of ultimate success. Consequently, we hypothesize that victorious initiator governments gain the most benefits—victorious targets of aggression not doing as well as victorious initiators in terms of regime survival. Still, winning is better than losing; and this is expected to be true whether one is an initiator or the target.

We propose, then, the following hypotheses which, if supported, provide an empirical foundation for the claim that the political welfare of a regime is intimately tied to the foreign policy welfare of the state. Data supportive of these hypotheses provides evidence of an important linkage between domestic affairs and foreign policy choices, providing an important contrast to the classical, realist perspective:

HYPOTHESIS 1. *The chances of a domestically instigated, violent regime change increase with defeat in war.*

HYPOTHESIS 2. *Violent regime change is least likely for winning initiators; the likelihood of violent regime overthrow increases for winning targets, losing targets, and losing initiators in that order.*

HYPOTHESIS 3. *The chances of a violent regime change increase with the costs of war, irrespective of the nation's war outcome or initial conditions.*

We examine whether during a war or after its conclusion the leader *and* his or her *entire regime* is removed from office through violent means. By violent means we intend to cover revolutions, coups d'état, and the like but not (as noted) removal by the opponent state. Removal of an entire regime is a very drastic act that is not always easily accomplished and that when carried out successfully, often requires appreciable political mobilization. If regime change is a regular feature of major failures in national security policy, then the *expectation* of such a consequence is probably an important, domestically induced, pacifying element in international affairs, an element that helps account for the frequency with which states tolerate one another, rather than wage war (Keohane 1984).

For removal from office through violent means to be reasonably related to war experience, it should occur within a period from after the war's onset to a relatively short time after the end of the war. We define two alternative time frames as the relevant periods for consideration: (1) the period from the beginning of the war until three years after its end, and (2) the period from one year before the end of the war until one year after its end.

For our purposes, the first period, extending up to three years beyond the end of a war is taken as the germane, *extended* time frame for a war-related violent regime change to occur. We choose a three-year interval because it offers the opportunity for war costs and outcomes to be recognized by the population and for an opposition to form and act. Taking it further than three years, however, could possibly let nonwar phenomena enter the picture more strongly. If a leader's regime is removed during the war or within three years of its termination, this seems to us as a reasonable case of war-related removal.

Two questions arise regarding this extended time frame. First, why did we use the entire period of the war? It is reasonable to suspect that policy failure will not necessarily be evident during the entire period of the war and that therefore we should look only at the period around the end of the war. For example, policy failure would not have been evident to Germans in 1940 but had become manifest by late July 1944. The problem with this is that we know of the end of the war post hoc. Those who forcefully overthrow a regime during wartime to change governmental policy have no way of knowing when the war will end. Second, why do we choose to extend our data into the third year after the end of a war? Do we not run the risk as the years pass beyond the end of the war of including violent regime changes having less and less to do with war experience and more to do with other possible variables? The answer to this is, of course, *yes*. However, any decision on a time frame will necessarily be arbitrary. We believe that an ample enough amount of time needs to pass so that the costs of a war can become apparent. After the end of the war but before fours years have passed, these costs are most likely to affect the victors. Thus, the *time period will likely make the test of some of the* hypotheses more stringent. The extended time frame has the virtue that it is unlikely to exclude relevant violent regime changes, but it has the limitation that it may include changes that were not related to the proximate war experience.

As a check on this extended time period we also evaluate our hypotheses in terms of a more *constrained*, second time interval encompassing violent regime changes that took place in the period from one year before the end of the war to one year after its end. In this way we can better evaluate the robustness of the evidence we uncover and assess whether it is consistent or inconsistent with our hypotheses. With this narrower time frame we mitigate the risk of including "irrelevant" regime changes; but, of course, we increase the chances of excluding violent regime changes that should have been included. Thus, each of our time frames offsets a limitation of the other, providing a more general basis for evaluating the reliability and robustness of the evidence we report.

THE DATA

Our data are relatively straightforward. The war data are taken from the well-known collection of the Correlates of War project reported in Small and Singer's (1982) *Resort to Arms.*[3] The data set not only reports on national involvement in all international wars between 1816 and 1975 with at least one thousand battle-related fatalities but also identifies the nations that initiated the wars and the eventual winners and losers.

We eliminate two categories of cases. First, we drop the cases in which the regime change is forced upon a state by its external enemies during the war or at its conclusion. This is consistent with our interest in domestically instigated violent regime change. For example, we do not include the new German governments that emerged in the years following the end of World War II or the removal of the Dutch government by the Germans during the war. Second, we exclude the cases in which there was no clear-cut winner (e.g., the Korean War). After removing these, our data set consists of 177 cases.

The data on violent changes of regime were derived from two basic sources. First, Banks's *Cross-Polity Time-Series Data* lists, by year, national attributes with respect to type of regime and number of successful forceful changes of government between 1815 and 1965 (1971, 3–53). These data were checked against the historical chronology given in Langer's (1952) *Encyclopedia of World History* and were also compared to information contained in Spuler's *Rulers and Governments of the World* (1977). Post-1965 data were gathered from *Facts on File* and Bienen and van de Walle's (1991) collection of data on political leaders.[4] The central question we asked was, Did there occur during a war or within the first three years after the end of a nation's involvement in an international war *a forceful, irregular, domestically instigated* change of its governing regime that resulted in the replacement of one elite by another? The identities of the cases of violent regime change we record are given in the Appendix.

In order to test Hypothesis 3, we need data on the costs of war. Measuring the costs of war directly is not a simple matter. The price of a war includes not only the immediate financial costs but needs to reflect opportunity costs as well. Although Organski and Kugler (1980) have been able to calculate some important dimensions of war costs for a few nations, we know of no data set that provides us a readily available measure for the number of nations with which we are dealing. However, an alternative measure is accessible in the war lethality data contained in the Small and Singer compendium (1982, 82–95). Small and Singer list for each nation's war participation the number of months it participated in the war, its number of battle deaths, its total population, its battle deaths per nation-month and its battle deaths per ten thousand population. This last measure is particularly attractive, since it is consistent across time and is insensitive to the size of a nation's population. Using battle deaths per ten thousand population permits us to make comparisons across nations and ask whether on average the winning targets who experienced violent regime changes suffered more deaths relative to their population than did the losing targets. Because these data are highly skewed at 11.98, we used a logarithmic transformation to reduce skewness considerably, to .95. In addition, it makes substantive sense to use the transformation because increasing battle deaths probably have a decreasing marginal impact that would otherwise be exaggerated (Jackman 1991).

DATA ANALYSIS

At the outset it is worthwhile to note that of the 177 war-participating nations in our data set, 32 (18.8%) experienced a violent change in their regime during the war or within three years of the war's end. It is difficult, however, to know how to evaluate this figure without some base against which to compare it. Is this 18.8% rate higher or lower than that experienced in nations that did not participate in war?

In order to estimate the effect of war on the rate of violent regime change, we drew a random sample of nation-years between 1815 and 1975, excluded any cases already in our war involvement data set and then determined whether the remaining nations experienced a violent regime change in the year drawn or in any of the three subsequent years.[5] We performed this procedure three times. The percentage rates of violent change in these samples were, from lowest to highest, 8.91%, 9.92%, and 10.21%.[6] The rate shown in the data of 18.8% indicates that war participation itself approximately doubles the chances of the regime's being violently overthrown by domestic opponents as a consequence of war participation.[7]

Our hypotheses, to which we now turn, predict significant variation across this base rate. With respect to hypothesis 1, what is the effect of winning or losing a war on the fate of the regime? A cross-tabulation of the data shows that there is a relatively strong relationship between losing a war and drastic changes in political leadership, with 29.5% (23/78) of the losers, and only 9.1% (9/99) of the winners, experiencing an violent regime change ($x^2 = 12.25$, $p < .01$). This is consistent with our first hypothesis and with the notion that failed national security policies provoke domestic political retribution.

Hypotheses 2 and 3 may be tested much more rigorously, since their predictions lead us to anticipate some relatively sharp differences in the data. Hypothesis 2 indicates that victorious initiators experience the lowest risk of violent regime change (i.e., in the probit analyses the relevant coefficient, b_1, is expected to be negative). The signs of the coefficients associated with the remaining variables from Hypothesis 2 (i.e., b_2, b_3, and b_4) can be positive or negative. In either case, b_2 and b_3 are expected to have magnitudes that fall between b_1 and b_4. This indicates that victorious initiators are less likely to suffer from violent changes in regime than are any other type of nation with a recent war experience, while victorious targets are less likely to suffer violent regime transfers than are defeated targets; and both are less likely to have such an experience than defeated initiators. Hypothesis 3 states that the likelihood of a violent change in regime increases as the costs of war increase, thereby leading to the prediction that b_5 will be positive.

In order to ascertain the overall relationship of these variables to violent regime change and to each other, we construct two probit models containing these ordered inequalities. The first model contains the effects postulated in Hypothesis 2 on the likelihood of a violent change in regime, and the second adds the costs of war to these variables. The expectation from the probit analysis is that violent regime change equals

$$- b_1\text{init. wins} \pm b_2\text{targ. wins} \pm b_3\text{targ. loses} + b_4\text{init. loses}.$$

Adding costs, it equals

$$- b_1\text{init. wins} \pm b_2\text{targ. wins} \pm b_3\text{targ. loses} \pm b_4\text{init. loses} + b_5\log(\text{battle deaths/population}),$$

where $b_1 < b_2 < b_3 < b_4$.

Table 1 displays the results of these probit models. The first column of the table shows the results without the inclusion of war costs. Here, everything is consistent with our expectations as to whether a regime endures more than three years beyond the end of a war or is violently removed in response to the nation's performance in war. The coefficients are all as predicted, with $b_1 < b_2 < b_3 < b_4$ in each probit analysis.[8]

How much of a difference do these factors make in the violent removal of a regime? Table 2 reports the cumulative normal distribution of the coefficients shown in Table 1, column 1; these are the translation of the probit coefficients into statement of probability of regime overthrow, given the contingent condition identified.[9] Thus, for losing initiators, the increment in the probability of violent regime change is .44. The other relevant incremental probabilities show losing targets half (.22) as likely to be violently removed as losing initiators and winning targets half again (.11). For winning initiators the probability is exceedingly small (.008).

As shown in Table 1, column 2 when the costs of war (in terms of the log of the battle deaths per ten thousand population) are added to the probit, the coefficients maintain the same order shown in column 1 and are fairly close to their original values. (However, note that the coefficients for the winners move closer together.)[10] It is tempting to look at interactions between battle deaths and the other variables, but there is a more revealing way of getting at the question.

Drawing upon the same method used to derive the results in Table 1, Figure 1 is a graph of the effect of levels of battle deaths on the probability of violent regime change given the four contingent conditions of interest. For the winners, as battle deaths increase, the chances of being violently overthrown nearly double. For losing targets, the chances of removal increase from slightly over .40 to just under .60; for losing initiators, the probability moves from just over .60 to slightly under .80. When compared to the effects of the contingent conditions themselves, it is evident that the effects of battle deaths are far from insignificant and increase the probability of removal by substantial amounts. For example (and most sig-

TABLE 1

Violent Changes of Regime: The Effects of Winning and Losing, Initiation, and Battle Deaths for the Extended and Constrained Models

INDEPENDENT VARIABLE	EXTENDED MODELS (N = 177) (1)	(2)	CONSTRAINED MODEL (N = 171) (3)
Initiator wins	−1.26**	−1.16**	−1.27**
	(.351)	(.364)	(.393)
Target wins	−1.09**	−1.13**	−1.15**
	(.345)	(.349)	(.395)
Target loses	−.64*	−.57*	−.43
	(.311)	(.320)	(.333)
Log(battle deaths/population)	—	.14*	.19**
		(.072)	(.078)
Constant[a]	−.13	−.51*	−.73*
	(.242)	(.306)	(.328)
$\bar{x}^2$ ($p < .001$)	17.03	21.26	23.66

[a]The constant is the baseline established by the cases of defeat for the initiator.
*$p < .05$, one-tailed.
**$p < .01$, one-tailed.

nificantly), battle deaths increase the probability of winning initiators being deposed violently from .008 to just over .20. However, it would be a serious mistake to focus only on the differences between this figure and the data in Table 2, because there is another aspect of the data in Figure 1 that merits discussion. Note that even when battle deaths are included, the contingent conditions remain of overwhelming importance. Thus, although the regimes of losing targets experience an increase in the probability of violent removal from about .40 to just under .60, the latter figure is *still below the starting point of losing initiators*. The same pattern holds in comparing the losing targets to the winning initiators and winning targets. In short, increasing battle deaths raise the probability of violent regime change; but the conditions specified in Hypothesis 2 are even more important.

The results of this analysis provide strong support for the hypothesis we originally offered about the effects of war outcomes on the political fortunes of the regimes that are the initiators or targets of international war. Clearly, regimes that initiate wars and do not prevail are at the highest risk of being replaced, while those who initiate and win are at relatively little risk. Our results with respect to the targets of international war are also as expected. All of this is true whether the costs of the war are included or not.

TABLE 2

Cumulative Normal Probabilities of Violent Regime Change

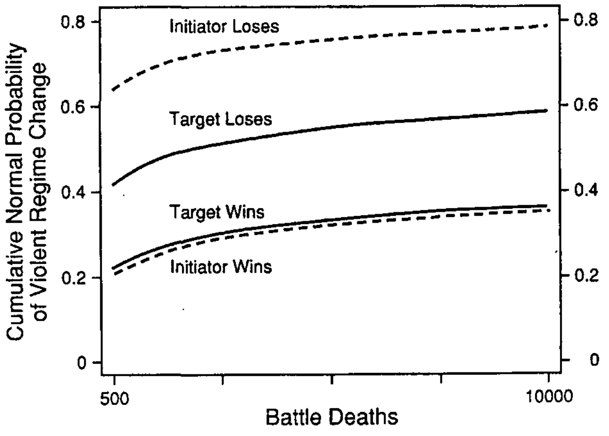
COEFFICIENT	CUMULATIVE NORMAL PROBABILITY
Initiator wins	.008
Target wins	.11
Target loses	.22
Initiator loses	.44

THREATS TO VALIDITY

Although our hypotheses are well supported, we pause to explore several threats to the robustness and validity of the results. Recall that the period over which we collected data on violent regime change extended from the onset of the war to three years beyond its end. As noted, it is reasonable to be

FIGURE 1

War Performance and the Probability of Regime Change



curious about the extent to which our findings would continue to obtain if this period were constrained to a much shorter one. For this purpose we remove from the data set all cases in which the violent change of regime took place more than a year before the end of the war and more than a year after the end of the war. This more limited time frame removes six cases from the data. The probit results from this more constrained data set are reported in Table 1, column 3. In terms of their sign, relative order, and statistical significance, the results shown here are extremely close to those obtained earlier, the main difference being that the effect on violent regime change of being a losing target is just beyond conventional standards of statistical significance ($p = .095$), although the odds are still better than 10 to 1 that the effect is not due to chance.

Another potential threat to the validity of our analysis derives from the fact that regimes and governments are always changing at some rate and that some nations are more prone to change than others. It is possible that to one degree or another, the changes we observe simply reflect a continuation of past governmental instability. After all, if a nation has experienced political instability in some period before its involvement in war, it is not unreasonable to think that irrespective of war involvement, some of that instability may be sufficiently embedded in the political system to account for the violent regime changes we observe better than does war involvement. To assess this possibility that the results are spurious, we constructed an index of prewar political stability for each of the 177 cases in our data. This was done by enumerating the number of successful coups d'état each case experienced in the 10 years prior to its war participation.[11] Interestingly, the results of this probit reveal that a relationship does exist between prewar political instability and violent regime change among the states participating in war (coefficient = .22, p = .027).

Given this, we need to assess the extent to which this estimated relationship is independent of the state's initial position in the war, its outcome, and its level of cost. It is not, after all, unreasonable to suspect that the regimes of unstable polities may be more sensitive to, for example, the costs of a war than regimes existing in generally stable environments. If so, then including our measure of prewar instability into our previous model could cause some of the already reported effects to be altered.

To investigate this prospective threat to the stability of the results, we include the prewar instability variable in the original full model of Table 1. Table 3 shows that the outcome and cost variables continue to have relatively the same estimated effects found in the earlier analyses even when the index of prewar instability is included. In other words, the political instability variable does *not* have a discernible impact on the weight of war outbreak, war outcome, or costs we estimated earlier. Our results continue to appear robust even when taking into account the impact of prior political instability.[12]

TABLE 3

The Effects of War Outcome, War Costs, and Prewar Instability on Violent Changes of Regime

INDEPENDENT VARIABLE	COEFFICIENT	STANDARD ERROR
Initiator wins	−1.26**	.380
Target wins	−1.16**	.353
Target loses	−.71*	.330
Log(battle deaths/population)	.15*	.074
Prewar coups	.27*	.125
Constant[a]	−.64*	.311

Note: N = 177, χ^2 = 25.91, f = .000.

[a] The constant is the baseline established by the cases of defeat for the initiator.

*$p < .05$, one-tailed test.

**$p < .01$, one-tailed test.

CONCLUSION

Our results demonstrate quite clearly that the outcome of war has a dramatic effect on the fate of the regimes involved. Our hypotheses set out the expected associations; and all were supported by the evidence with regard to irregular, forceful changes in regime. Although this relationship has not been previously established, some may not find the results surprising. However, what we have shown is not merely a relationship, but (1) an estimate of the *size* of the effect of war on violent regime change against a baseline and (2) the *specification and testing of quite specific, detailed hypotheses* describing the relationship of war, war costs, and regime stability to violent regime change.

Although broadly based in time and ranging across all types of political systems, the results are fully consistent with the claim that the political welfare of governmental regimes is directly tied to the performance of the nation in war. This conclusion casts interesting light on a controversial part of international relations theory. The evidence presented here can be taken as an evaluation of the claim that the political welfare of foreign policy makers is intimately tied to national performance in conflicts that pose a potential threat to sovereignty and that the preferences of such leaders are constrained so as to give highest priority to maximizing the state's overall welfare (Bueno de Mesquita 1981). Having observed that regimes are ex post punished for failed foreign policies, we propose an ex ante inference. In the context of models of rational action, behavior is strongly influenced by expectations about consequences of actions "off the equilibrium path"; that is, the choice of actions is shaped by beliefs about what would happen if some other action were chosen. Leaders can anticipate that they will be held accountable for failed foreign policy adventures. Consequently, the choice of war-related behavior is likely to be dampened by the fear that the regime will be punished if things go awry. Probably, the wars we do observe are cases where leaders generally thought the foreign policy action would turn out well from the perspective of their regime. They thought the potential benefits from fighting outweighed the associated risks to the regime. In that sense, the true effects of war on regime change are probably stronger than the observed effects reported here. The true effects include instances of wars that did not happen because of the anticipation of domestic political punishment. We believe, therefore, that the results lend considerable credence to models that treat war/peace decisions as if they are the product of rational choices by a unitary actor whose preferences are partially shaped by the existence of a threat to the state's sovereignty or autonomy and who takes into account the distribution of preferences and interests of the constituent elements that make up the domestic affairs of the state. The results show that governmental leaders must attend to the domestic political ramifications of their decisions in choosing foreign policy actions. The standard realist account, which largely ignores domestic political considerations, is difficult to sustain in light of the evidence adduced here.

APPENDIX

Our cases of violent regime change and year of war onset are derived from Banks 1971, *Facts on File* for 1966–75, and Small and Singer 1982.

Nation	Year of War Onset
Spain	1823
Mexico	1846
France	1849
Argentina	1851
Columbia	1863
Paraguay	1864
Spain	1865
Peru	1865
France	1870
Peru	1879
Guatemala	1885
El Salvador	1885
Honduras	1906
Honduras	1907
Nicaragua	1907
Turkey	1911
Turkey	1912
Germany	1914
Greece	1914
Bulgaria	1914
Turkey	1914
Austria–Hungary	1914
Russia	1914
Hungary	1919
Greece	1919
Turkey	1919
Paraguay	1932
Italy	1939
Syria	1947
United Arab Republic	1947
Pakistan	1965
Cyprus	1974

Notes

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1. We emphasize that the cases to be examined in the analysis are those in which the regime is removed by its domestic opponents. Thus, regimes that fall by virtue of foreign imposition are not included in the data.

2. Despite the time that has passed since Stohl's review was completed, it probably remains the best overall treatment of the relationship between internal and external conflict.

3. We include the wars listed in the Correlates of War data set (Small and Singer 1982). The wars do *not* include events that are interventions in which a government is removed forcefully by another without sufficient resistance being present to reach the "war" threshold of one thousand battle deaths. For that reason such events as the disappearance of the Baltic Republics into the Soviet Union in 1939 and the German invasion of Denmark in 1940 are not included in the analysis.

4. Also available to us were Gurr's Polity II data. These, however, were not fully suited to our needs. Although these data are quite extensive with respect to the measurement of certain kinds of political change, they focus not on the regime (i.e., the members of the political elite who make state decisions) but on the much broader, underlying patterns of political authority and constitutional structure. Our interest is in the fate of the group of political leaders who take a nation into war and not necessarily on the overall pattern of state authority, although several such changes are recorded in the data. Thus, for example, although there have been many irregular, usually forceful, changes of regime in Mexico (Bienen and van de Walle 1991, report 28), the Polity II data record only four changes of polity. The Polity II data, it may be noted, also contain Banks's (1971) data on violent political changes on which we partially rely.

5. We chose this time period because the average length of a war in our data set was just over one year. Combining that year with the three years we allowed after the war ended yields a time frame comparable to that used in our analyses of forceful regime changes among states involved in war.

6. Excluding the nations in our data set for the years they fought and the three years after the war produces a data set of slightly more than three thousand nation-years of data. We drew 5% samples from these. While our data go to 1975, 10 years beyond the end of the data from which the samples were drawn, it is not clear that there were any marked differences in these omitted years.

7. Almost two-thirds of the violent changes of regime in the three samples are to be found in the nations of Latin America. This high rate of forceful regime change in Latin America is consistent with the findings of Bienen and van de Walle (1991, 86). It might be argued that the samples should be stratified by region to reflect the rate of change more accurately. Such a procedure would undoubtedly show that the effect of war participation on relevant regime changes in the non–Latin American nations is much *higher* than we show here. However, given the state of the data set from which the samples were drawn (i.e., no geographic code is present), it is difficult to implement this method. Moreover, it is unnecessary to our purpose, which is only to show that war participation makes a meaningful difference in the rate of violent regime change. We will approach another aspect of this question further on.

8. Because of the nature of the independent variables, it was necessary to leave one of them out of the estimation of the model. The effect of the omitted variable is then contained in the constant. However, to make sure that this procedure was not favoring our hypotheses, we computed the zero-order product moment correlation between violent change of regime and all the variables in the probit reported in Table 1, column 2. The results are perfectly consistent with the esti-

mated model and our hypotheses. The Pearson product moment correlations between violent regime change and our variables are *initiator wins* −.22, *target wins* −.06, *target loses* .01, *initiator loses* .33, and *log(battle deaths/population)* .27.

9. For a very accessible explanation of this technique, see Wolfinger and Rosenstone 1980, app. C.

10. We provide the cross-tabulation of predicted and actual outcomes based on the probit analysis shown in column 2. The model of regime turnover that we propose is not a full equilibrium model. As such, it does not take into account all of the factors that contribute to forceful, domestically instigated regime change. Consequently, the model is not expected to account for all—or even most—of such regime changes but, rather, to isolate conditions that systematically *increase the likelihood* of such change. The model's significance is best evaluated, then, in terms of the extent to which the probit coefficients are consistent with the expectations expressed in our hypotheses, rather than in how many cases were fully determined by the factors we address in our model. The cross-tabulation follows (pseudo R^2 = .11). The results are consistently robust in terms of the ordinality of the coefficients; the differences across coefficients approach but do not achieve significance.

	Predicted Outcome	
Observed Outcome	No change of regime	Change of regime
No change of regime	143	2
Change of regime	26	6

11. Data on successful coups d'état were taken from Banks 1971, segment 1, field E and (for years after 1966) from *Facts on File* for 1966–75. We would have preferred to use data on attempted coups d'état, as well as successful ones, but we were unable to find any broad data reporting such attempts.

12. Our analysis has treated all regimes as equivalent; but we know that they differ, among other things, with respect to the ease with which leaders may be replaced. Elites in some states may be forced to use violence to remove the regime because other alternatives are not readily available to them, while other states (i.e., democracies), may have well established, even institutionalized, methods for removing the political leadership. Further, the number of winning democracies in our data may distort some of our findings. To investigate this possibility, we estimated the probit contained in Table 1, column 3 after removing democracies. Although there are some modest changes in the coefficients (none larger than .15), the order of the coefficient values remains the same.

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REVOLUTIONARY COLLECTIVE ACTION AND THE AGENT–STRUCTURE PROBLEM

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Unraveling the nexus between agents and structures is fundamental to an understanding of political and social change. The two most prominent methodological approaches to explain revolutionary collective action involve either individual reductionism or structural reductionism. Both approaches result in theoretical inconsistencies and/or explanatory anomalies. An alternative proposed here utilizes the concept of framing *developed in behavioral decision theory primarily by Quatrone and Tversky. It directly addresses the agent–structure problem by developing the proposition that individuals evoke alternative decision rules in different structural contexts. The result is greater theoretical coherence and resolution of anomalous cases. Additionally, this model begins to define a new role for ideology in explanations of revolutionary collective action.*

A little over a decade ago the study of revolution took two, distinctly separate, theoretic turns. In *States and Social Revolutions* Theda Skocpol (1979) argued for a structural and nonvoluntarist study of revolutions. At the same time, in *The Rational Peasant*, Samuel Popkin (1979) argued for a study of peasant revolutionary action based upon the axioms of rational choice. Both studies contribute to our understanding of revolution but are in an important sense methodologically—and thus theoretically—incompatible. Skocpol's exclusive focus upon social structures and the Popkin's equally exclusive emphasis upon individual action means that each implicitly denies the legitimacy of the other. Since their initial writing, these works and their attendant methodological approaches have remained distinct and apart.

This methodological bifurcation reflects a larger theoretical dilemma in the social sciences that Wendt (1987) refers to as the *agent–structure* problem. Political scientists, in the attempt to understand empirical reality, are everywhere confronted with two "truisms" about the social world. First, individuals are often agents whose intentional, self-conscious, actions both reproduce and transform social reality. Second, society consists largely of interconnecting social relationships (structures) that condition the interaction between agents, as well as the outcomes of agent action. Combined, these two dynamics suggest that a major theoretical task for political science is to unravel how agents and structures interact, thereby approaching more comprehensive explanations of social change.

The most common method employed to cope with the agent–structure problem has been to impart to either agents or structures a fundamentalist property. Such reductionism is evident in Skocpol's analysis, for she views individual revolutionary action to be a function of social-structural dynamics. Similarly, Popkin views social change (revolution) as a function of intentional maximizing behavior on the part of individuals. In neither approach is the agent–structure problem resolved: it is metatheoretically denied.[1] A second approach not often employed in political science but growing in popularity is to impart to both agents and structures equal ontological status and to view agents and structures as mutually constituted (Giddens 1984). However, without any meaningful analytical separation between agents and structures, it becomes impossible to unravel the causal nexus between the two and thus locate the cause or causes of social change (Taylor 1989).

In this vein, James Coleman (1986) argues that any theory of society that attempts to tackle the agent–structure dilemma must be grounded in an explicit model of individual purposive action. Harkening back to the writings of Hobbes, Smith, Locke, Rousseau, and Mill, Coleman notes that a model of purposive action was fundamental to classic political theorizing because

> it allowed connecting intentions of persons with macrosocial consequences. Thus the functioning of society as well as the engine of social change could be grounded in the purposive actions of individuals, taken in particular institutional and structural settings that shaped the incentives and thus action. Social theory with this kind of grounding made possible a conception of how social systems might be shaped by human will. (p. 1310)

The analysis that follows is so grounded. But in order to understand revolutionary movements, the nexus between individual action and social structure must be more completely developed than even Coleman's statement implies. I shall examine Skocpol's *States and Social Revolutions* as an example of methodological structuralism, then Popkin's *Rational Peasant* as an example of rational choice. It will be demonstrated that, upon close scrutiny, theoretical inconsistencies emerge in both approaches because of an inattention to the agent–structure problem.[2]

Against this, I will suggest one way in which these methodological (and as a result, theoretical) problems can be overcome.[3] I will lay the groundwork for a model of peasant revolt that makes explicit the connection between structural context and individual decision rules by expanding the concept of "framing" developed in empirical behavioral decision theory. My thesis here is that revolutionary movements can only be understood by addressing the agent–structure dynamic and that this, in turn, requires an understanding that individuals invoke different decision rules in alternative structural contexts. These rules are related to agent understandings about current and past structural relationships. It will then follow that the role of both revolutionary organizations and their ideologies is communicative, intervening between structures and agents. The result is a reintegration of ideology into a theory of revolution. Finally, I will discuss theoretical implications and possible research programs.

METHODOLOGICAL STRUCTURALISM

Skocpol's study is in large part a forceful and explicit defense of methodological structuralism. At the outset Skocpol states that the study of revolutions requires that the social scientist "take an impersonal and nonsubjective viewpoint" (1979, 18). Methodological individualism, which makes revolution a consequence of intended action, is discounted because it (1) incorrectly implies that social order rests in consent and (2) is empirically incorrect. Historically, revolutionary actors have taken advantage of structural crises, but intentional action in no sense generated them. Hence, individuals should be viewed as "participants in complex unfolding of multiple [structural] conflicts," rather than purposive actors. (p. 17). Thus an understanding of revolutions requires one to rise above the viewpoints of potential agents to find important regularities across cases (p. 18). For Skocpol such regularities are to be found in the commonalities between social structures.

Nonetheless, there is in Skocpol's description an implicit and theoretically necessary model of intentional peasant action. Indeed, it is because this model is not made explicit at the outset that the nexus between individual action and social structure is never fully developed. As a result, the theory (and its internal logic) fails to provide an effective explanation of peasant rebellion.

Skocpol begins the analysis by making two assertions about the nature of peasant revolt. First, peasants are in a permanent revolutionary condition. The peasantry is perpetually subject to economic, political, and cultural marginality and socioeconomic immobility. By definition, then, peasants "always have grounds for rebellion against landlords, state agents, and merchants who exploit them" (1979, 115). Peasant grievances—for more land, greater autonomy, lowered taxes—are thus inherent to, and defined by, their position as a social class. "What is at issue," says Skocpol, "is not so much the objective potential for revolts on the grounds of justifiable grievances. It is rather the degree to which grievances that are always at least implicitly present can be collectively perceived and acted upon" (ibid).

The potential of the peasantry to act collectively is a function of structural positioning. To revolt, the peasantry must possess a degree of leverage against the landed elite that exists only when there is sufficient peasant solidarity and community autonomy, that is, *tactical freedom*. Such conditions are usually attendant to rentier agrarian systems where local peasants work small plots of land and are free from strict supervision by the landed elite (1979, 116). Here, the tactical freedom afforded to peasants is more amenable to local organizing, since individuals are not pitted against each other competing for seasonal employment as wage laborers. It is important to note that as a necessary component, tactical freedom need not be inherent to the existing structural status quo. It can (as Skocpol's description of the role of the Chinese Communist party in the revolution suggests), in effect, be "created" by a revolutionary organization that expertly secures areas of local control in which solidarity and organization can flourish (p. 148).

Additionally, tactical freedom must be accompanied by a relaxation of effective coercive state sanctions that normally crush peasant revolts (Skocpol 1979, 115). (Actually the source of sanctions is not theoretically important. In Skocpol's structural framework it becomes central only because, since the landed elite is dependent upon the state as the primary sanctioning agent, state impotence translates into a collapse of effective sanctioning.) It is the collapse of coercive restraints that affords peasants the room to utilize tactical freedom in concerted attacks against the landed elite. The implication throughout is that these structural conditions are necessary and sufficient conditions for widespread peasant revolt.[4] By combining the notion of a class in a permanent revolutionary condition with necessary structural conditions, one can state a unified hypothesis concerning peasant revolt, namely, that given sufficient solidarity and autonomy, a precipitous decline in sanctions will spur peasant revolt.[5]

Now, even the strictest structural interpretation would grant that revolt is an aggregation of individual decisions; for peasants must ultimately themselves choose to revolt. The assertion made by structural theories like Skocpol's is that the nature, scope, and timing of such a decision is structurally determined by the position of the peasantry as a social class. As was just noted, Skocpol holds that class grievances are "collectively perceived and acted upon." Across cases, then, the model requires that peasants act consistently when confronted with similar structures, that is, that peasants employ a single and mutual decision rule.

For Skocpol's theory, the model of action can only be that when confronted with a revolutionary choice, peasants employ a decision rule akin to a microeconomic rational maximizer and are thus driven by the desire to secure maximum benefit. Recall that given tactical freedom, the understanding that sanctions will be swift and sure prevents peasants from acting collectively on their structurally determined and class-inherent desire for improvement. In short, peasants weigh the potential benefits of rebellion against the perceived costs (likelihood of retaliatory sanctions). When sanctions are understood as being swift and sure, the cost of rebellion is too high. However when sanctions are ineffective or impotent (as is the case in the administrative state crisis Skocpol describes), the decisional calculus changes, and rebellion becomes feasible.

The notion that a model of peasants as rational maximizers is central to Skocpol's theory is further supported by the explicit denial of alternative motivations. Peasants are not mass-mobilized by fundamentally new goals, values, or system-transcending ideologies (1979, 114). Peasant activities in revolutionary times and in mere rebellions are not fundamentally different (ibid). Both are directed at achieving immediate and local goals, such as greater access to land and relief from high taxes on their surpluses. Peasants participating in revolts are not swept up into a revolutionary fervor; rather, they are characterized as pursuing latent, class-based desires and demands in a more permissive structural environment. The point here is not only that the rational maximizer model of peasant action is implied throughout but that it is also a necessary assumption if the structural hypothesis is to work. It is only within the context of such an assumption that it can be argued that structural conditions "lead" to revolt.[6]

However, there are good theoretic reasons to expect that this nexus of rational egoism and permissive structures will not result in revolt. Here, an understanding of free-rider logic is essential. When confronted with the decision of whether or not to contribute to the provision of a public good (such as revolution), a rational individual will choose to free-ride rather than contribute a fair share. As Mancur Olson puts it: "The notion that groups of individuals will act to achieve their common or group interests, far from being a logical implication of the assumption that the individuals in a group will rationally further their individual interests, is in fact inconsistent with that assumption" (1965, 2). The assumption of rational maximization as a decision rule on the part of individual peasants does not logically lead to the conclusion Skocpol's analysis assumes, namely, that peasants will collectively act to address their class-defined goals and aspirations. Thus, while the structural hypothesis directs itself to explaining the timing and scope of peasant revolts, the individualist assumptions implicit throughout would seem to argue for a nonevent.

In fact, there are historical instances in which, given the necessary and sufficient structural conditions as described by Skocpol, peasants choose not to revolt. Ronald Waterbury (1986) in a detailed empirical analysis reconstructs and compares the economic and social conditions across the states of Oaxaca and Morelos in Mexico during the Zapatista Revolt. Waterbury notes that both states were predominantly populated by rural peasants. In Morelos the traditional clientilist economic system had undergone changes that were the result of modernization and centralization—a manifestation of the attempt to increase sugar production. In this system, dominated by the modern hacienda, the peasantry as a class had little or no autonomy in local decisions, owned very little land, and was forced to sell labor as a primary income source. This structural arrangement left peasants in direct competition with each other for work and land. They did not possess the tactical freedom with which to organize collective action. Conversely, Oaxaca did not undergo modernization; as a result, peasants enjoyed much local autonomy, owned a significant amount of land, and possessed a strong sense of community solidarity.

Skocpol's structural theory would predict that during the period of virtual state collapse in the early 1900s Oaxacan peasants, who were structurally positioned to take advantage of decreased state sanctioning, would openly revolt. However, Waterbury describes the Oaxaca response to the Zapata Rebellion as "reactionary," or at best neutral (1986, 411). Peasants opposed Zapatista leaders, who were ostensibly fighting to improve their condition. Interestingly, Morelos, with its system of modern haciendas and almost complete local control over local peasant life, was visited with active and violent rebellion.

The main thrust of this description is not to challenge the conclusions Skocpol draws about the importance of a structural analysis in determining the susceptibility of specific types of regimes to revolution and conditioning its eventual, often unintended, outcome. On this score, structural analyses are fundamental. Rather, the suggestion is that an exclusive focus upon structural dynamics had led to an incomplete development of a necessary nexus between structure and action and that as a result, the theory cannot account for the widespread peasant revolt against landlords, which Skocpol considers fundamental in explaining larger social revolutionary transformations.

While Skocpol's account displays the greatest theoretic rigor, other largely structural accounts have not completely ignored individual motivations. For example, Hooglund (1982), Tutino (1986) and Walton (1984) all focus primarily upon the deleterious effects of structural transformation on peasants (land reform, market penetration, subsistence autonomy, and security crises, etc.). These transformations, it is argued, lead to a change in peasant outlook that in turn, accounts for revolt. However, in agent–structure terms, the model of individual action is lacking. Walton emphasizes "noninevitability," Tutino focuses upon the shift from resignation to moral outrage, and Hooglund highlights the decay of regime legitimacy. These (new) peasant perceptions, while related to the structural dynamics described, can not adequately confront the free-rider critique.[7] Again, the desire for collective action is, by itself, inadequate.

METHODOLOGICAL INDIVIDUALISM

In an extensive analysis of peasant collective action in Vietnam, Popkin argues that "participation in revolution can be seen not as an anachronistic attempt to restore a fictional golden age, but rather as a calculated effort to improve one's future position" (Perry 1982, 890). The fundamental analytic concern is not the extent of the threat to a social class but, rather, the level of risk to the individual (Popkin 1979, 245). In contrast to Skocpol, Popkin begins at the individualist end of the methodological spectrum by asserting at the outset that peasants, in virtually all of their activities (economic, political, social, familial) employ a self-interested rational maximizing decision rule. Specifically, "individuals evaluate the possible outcomes associated with their choices in accordance with their preferences and values. In doing this they discount the evaluation of each outcome in accordance with their subjective estimates of the likelihood of the outcome. Finally, they make the choice which they believe will maximize their expected utility" (p. 31).

In focusing upon individual intentional action, Popkin is rejecting the claim that social structures should be the central analytic variable. For example, a structurally generated collapse of effective peasant sanctioning (central for Skocpol) is not an important variable, because "it must be stressed that village officials and landlords had to be undermined. Their power had not 'decayed' during the colonial period. . . . Each of these movements had to build a power base while opposed by rural and economic elites, who had been strengthened, not weakened by colonial policies" (1979, 187). Seemingly, social-structural conditions have no meaningful impact upon the potential of such groups to organize the peasantry effectively. The aggregation of intentional individual actions, rather than permissive structures, accounts for revolution. Concomitantly, the objective condition of the peasantry as a social class—its position in the social structure—is unimportant. For Popkin, peasants are not permanently revolutionary, since there is "no clear relationship between substance threat (or decline) and collective response" (p. 245). Rather, the decision to act is a function of the ability of revolutionary organizations and the political entrepreneurs who lead them to provide and create incentives that induce participation. Through the strategic manipulation of collective goods, political leaders entice rational egoistic peasants to provide support for revolutionary organizations. Here, Popkin begins where my earlier account of Skocpol ends, that is, with an explicit acknowledgement of the impediments to successful collective action (free-rider logic), which Skocpol does not address.

Under the heading "The Political Economy of Revolution" Popkin identifies four conditions under which a peasant would contribute to a revolutionary movement. Initially, Popkin is forced by his own findings to acknowledge the existence of moral compulsion as a motivation but then dismisses it by stating that "situations in which [such] contributions are widespread are infrequent in peasant society" (1979, 254). Next, Popkin argues that peasants participate when (1) it is rational in cost–benefit terms, (2) there are sufficient selective incentives, and (3) the contributions of others are contingent upon one's own (ibid). While these remaining three conditions account for contributions in a context of, say, interest groups (as in Olson's original formulation), they are problematic when applied to the large-scale social pressures of revolution. Each will be discussed in turn.

Cost–Benefit Calculus

Little is required of a revolutionary organization whenever, from an individual's perspective, it pays to contribute in simple cost–benefit terms. Conceptually, there are two instances in which this is the case. The first requires that the benefits of participation flow only to those who contribute; by definition, such a good is "excludable." Popkin demonstrates that in the Vietnamese case, the emergence of support for revolutionary organizations as such was largely a function of their ability to organize peasants into projects addressing their local and immediate concerns and where the benefits could be selectively allocated to those who contributed to the project (insurance schemes, the raiding of granaries, etc.). However, the notion of revolution implies systematic reorganization. In this regard, revolutions approach a "pure" public good that is by definition a good from which individuals cannot be excluded. Additionally, Popkin cannot account for individual revolutionary choices where the dominant incentive is to defect. The same dilemma that confronts Skocpol's analysis reemerges here. Class or group rationality that would imply revolt is not consistent with individual rationality and the incentive to free-ride.

By the second variant of simple cost–benefit calculations, rationality would dictate participation if the costs of defection outweighed those of contributing to the revolutionary effort. While Popkin himself only mentions this theoretical possibility in passing, Michael Taylor, expanding upon Popkin's work, argues that membership in a village community itself makes participation rational. Taylor emphasizes the notion that defection often has social costs:

> A number of studies have shown how important "social" incentives are in mobilizing people into collective action. An individual joins or contributes or participates because he is asked tacitly or overtly pressured by friends, colleagues workers or co-members of the association's local branch or cell. He cannot say "no" to them; he is afraid of losing their approval, respect or cooperation. (1988, 84)

When applied to revolution, however, such an insight is problematic in at least three ways. First, there is little data (none is provided by Popkin or Taylor) predicting that villagewide contempt would be sufficient threat to lead a peasant to incur the risks of engaging in open revolt—which is often associated with increased probability of death. Second, and still more fundamental, Taylor's argument assumes a community already committed to revolution when the individual peasant is confronted with a revolutionary choice; that is, there must be some threshold (number of peasants) of community commitment below which ostracization becomes ineffective. Hence, while an individual in an already-committed village might be swayed by social pressure, this explanation cannot account for revolutionary choices up to $N - 1$, where N is the minimum level of commitment required for social pressure to be effective. Finally, a reliance upon the village as a key variable explicitly acknowledges the importance of accounting for social structure.[8] It depends, for example, upon the nature of relations within the local community, with other communities, and between the community, landlords, and state officials (Taylor 1988, 68).

Selective Incentives

The next condition in which revolutionary collective action is possible involves, in effect, decisions not related to revolution at all. Here, Popkin invokes the *by-product* theory of collective goods. When a collective good is the by-product of an organization's primary function and this primary function is the source of organizational support, it is rational for an individual to contribute if the benefits of this primary function flow exclusively to those who contribute.[9]

Here again, the nature of revolutionary collective action, which is often long and protracted, makes this description problematic. For the American Medical Association to lobby Congress, nothing more is required of doctors besides the payment of dues. The problem is that revolutionary struggles require increased popular action—and therefore contributions—above and beyond that which was initially sufficient for the organization to emerge and prosper. Examining village mobilization by the communists in Aman leads Popkin to note that organizing villagers into literacy campaigns, effecting improvements in water storage and irrigation facilities, and reforming the communal land system (including a more favorable tax system) were strategies directed at attracting peasant support away from rival organizations. However, successful as they were in garnering support and organizing the peasantry, these contributions per se can in no sense be directly translated into revolutionary action. Teachers' contributions of skill for the literacy campaign or farmers' contributions of labor to improve water delivery systems do not create protracted revolutionary war as a by-product. The question remains, Why revolt?

If, after a successful organizing campaign, peasants are asked to step up their contributions and actively revolt, they are in effect being asked to contribute to a new public good. This violates the conditions of by-product theory as stated by Olson (1965, 132–33); that is, the collective good of revolt is no longer a by-product of organizational support. Rather, individuals are now being asked to contribute *directly*, not indirectly, to revolution. Rationality here would dictate a separate calculation of the benefits and costs of revolution distinct from considerations or organizational support. As noted, in such a calculation the dominant incentive is to free-ride. The collective good of sustained revolutionary opposition as a by-product is unlike organizational lobbying by the American Medical Association, in that the former requires more of individuals than support for an organization.

Contingent Contributions

The final condition under which rational peasants would contribute to collective revolutionary action is that each individual must view a personal contribution as critical and necessary for the success of the overall collective project. This requires that the overall goal be broken down into "many small independent pieces, all of which are necessary," so that, since "each person has a monopoly over a necessary factor for the final goal, all contributions are essential" (Popkin 1979, 257). Here, peasants are acting as purchasers of private goods in the marketplace, with the very important stipulation that such a purchase will be effective only if "it is known for certain that all the others designated as contributors will contribute" (van de Kragt, Orbell, and Dawes 1983, 116). Such a stipulation, in turn, suggests that if individuals act "unreasonably" or if there is some reason to believe that others will not act rationally, the logic for an individual's contributing immediately collapses (p. 119). In revolutions, there are good reasons to believe that it will collapse.

As formally developed, investigators looking at how criticality of contribution impinges upon individual decisions to participate have assumed that the contributions required of each individual are essentially the same. Additionally, there is some preliminary evidence to suggest that individuals project their own expectations about cooperation to others (Dawes, Mctavish, and Shaklee 1977). Yet in the process of revolution, some tasks, by their very nature, are likely to demand more from some individuals than others. For example, helping in the construction of a dike entails less risk than engaging state forces in battle.

Therefore, Popkin is here burdened with the dilemma of demonstrating that the most difficult task (in terms of subjective estimation of costs) is seen by *every* individual as rational, so that noncooperation is not projected to others, and defection does not become the rational strategy. One might employ a class-based analysis and argue that the observed living conditions of a particular class or group are so bad that for all necessary tasks, cooperation is seen by all as rational. But this would directly counter Popkin's explicit denial of the need to look at social structure, group rationality, and the objective living conditions of the peasantry. Further, such a perspective would not allow the observer to account for individual decisions not to support a revolutionary organization by distinguishing between organizational failure and an a priori lack of necessary structural conditions. Thus, such a perspective is indistinguishable from a structurally deterministic explanation.

This argument implies that economic rationality alone inadequately accounts for sustained revolutionary movements. The model of action employed here was initially developed without consideration of the types of social-systemic dynamics central for an understanding of revolutions and then was applied to peasant revolt.[10] As a result, it is unable to provide viable predictions about revolutionary action, suggesting that this is a fundamentally different type of decision.

RISK-SEEKING CHOICES

If we are to understand peasant revolutionary action, then fidelity to Coleman's (1986) initial argument (that explanations of social phenomena need to be grounded in a model of individual action) dictates that we begin by reformulating the model of individual choice. Such a model must (1) identify what sort of decision rule is in effect (this, I shall argue, requires reference to social structure) and (2) demonstrate how this rule, when employed, accounts for revolutionary collective action.

Central to classic microeconomic rationality is the assumption of invariance. As Quatrone and Traversky note: "Perhaps the most fundamental principle of rational choice is the assumption of invariance. This assumption, which is rarely stated explicitly, requires that the preference order among prospects should not depend on how their outcomes and probabilities are described and thus that two alternative formulations of the same problem should yield the same choice" (1988, 727).

This assumption carries two important characteristics. First, decisions are outcome-oriented. They are based upon the utilities (weighted by their probability) that are the result of alternative choices. Second, the decision rule for a choice between two gains and a choice between two losses is essentially the same; both are evaluated in terms of movement along a linear and preestablished utility function.

As an alternative, *prospect theory* emerged from a branch of empirical investigation into the structure of individual choice that Simon (1985) has termed *bounded rationality*.[11] Results in this field suggest that individuals employ identifiable decisional heuristics that generate outcomes systematically deviating from the predictions of rational choice. Additionally, this research suggests that the choice of decision rule is contextually contingent.

Bernoulli (1954) initially altered the classic model and demonstrated that the relationship between money and utility is "concave"—consistent with the observation that a gain of $500 contributes less than twice the utility of a gain of $250. The concavity of the utility function predicts that individuals, in their choices, are *risk-averse*, defined as preferring a sure gain over a gamble offering a higher expected payoff. Subsequent research in the behavioral decision tradition added that the location of the status quo is important. Prospect theory makes a distinction between choices involving gains and choices involving losses and requires, therefore, the notion of a neutral status quo reference point (Figure 1). As Quatrone and Tversky put it, "According to prospect theory, an individual's attitude toward risk depends upon whether the outcomes are perceived as gains or losses, relative to the reference point" (1988, 722).

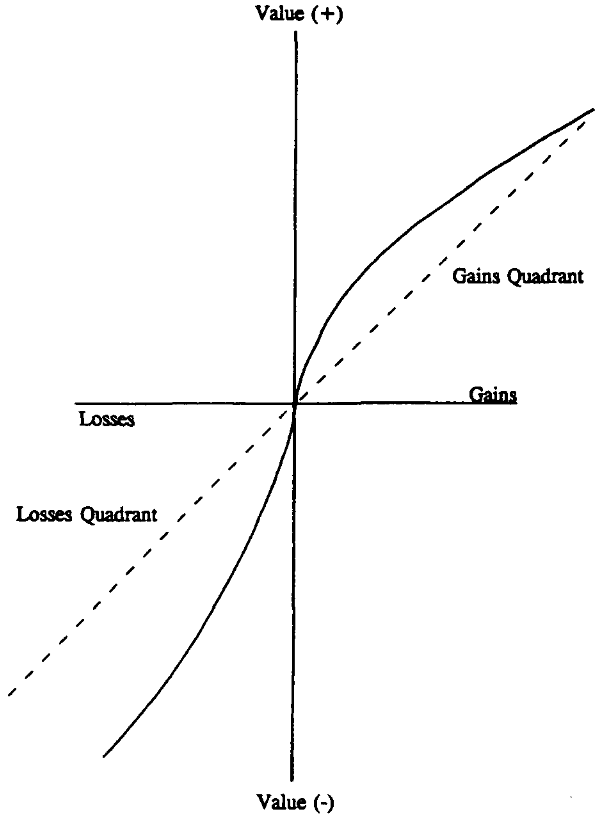
Here, the money-to-utility function in the gains quadrant (the upper right quadrant) is concave, consistent with Bernoulli's modification of the invariance model. The losses quadrant (lower left) is convex,[12] reflecting, for example, the observation that losing $500 does not seem twice as bad as losing $250. While individuals are risk-averse in the gains quadrant, the data show them to be risk-seeking in the losses quadrant.[13] The point here is that the decisional determinant is not the expected outcome, but how the choice is perceived relative to the status quo (reference point). Indeed, expected outcomes in one sense become irrelevant, since "the same decision can be framed in several different ways; different frames can lead to different decisions"—that is, "framing effects arise when the same objective alternatives are evaluated in relation to different points of reference" (Kahneman and Tversky 1982, 165). In this framework, explanations of individual choice are located in individual perceptions of gain or loss relative to the status quo.[14]

Both Popkin and (implicitly) Skocpol rely on the classic model of rational choice as a foundation for a model of individual action. If, however, the modified, status-quo-sensitive model is to move beyond a tool for criticism and contribute to a positive theory of revolutionary action, then a method is needed for determining when revolutionary decision becomes "framed" as a choice between gains or between losses. Accounting for revolutionary action requires conceptualizing a social-structural frame.

A *social frame* is defined here as the perception by individuals that participation in the existing social-structural arrangement means either gains or losses. However, in the standard explication of prospect theory, an individual who opts for the status quo garners no gains and incurs no losses. This initially appears problematic. However, there are at least two reasons why it is not. Adhering strictly to the framing effect as I have described it, the status quo will be placed in the losses frame if a deterioration of current conditions is expected to continue. Here, one who opts for the status quo can expect increased loss. Furthermore, there is no theoretical necessity that the reference point represent only current empirical conditions. Kahneman and Tversky have noted that "the reference point is the state to which one has become adapted" and that there are many cases in which "the reference point is determined by events that are only imagined" (1982, 171–172). Clearly, then, an individ-

FIGURE 1.

The Framing Effect



Note: The dotted line represents the original invariance model. The curve represents a modified, status quo sensitive, utility function suggested by the framing effect.

ual's reference point for any particular decision can be associated with some past state viewed as normal. Current conditions can be viewed as an "outcome" that is placed in the gains or losses quadrant relative to a subjective reference point that is itself reflective of past conditions.

In either scenario, this implies that peasants living through social-structural transformations that lead, for example, to an increased vulnerability to subsistence crises (market penetration into closed rural villages, regime change, famine, etc.) would view a choice in favor of status quo not as neutral but as a loss. For example, Eric Wolf argues that market penetration into rural villages damages the peasant class:

> The peasant economy was burdened by the pressure of taxation, by demands for redemption payments, and by the increased need for industrially produced commodities on the part of the peasantry itself. All together, these various pressures disrupted the precarious ecological balance of peasant society. Where the peasant had required a certain combination of resources to effect an adequate living, the pressures on these resources broke that ecological nexus. (1986, 175)[15]

The decision to revolt can now be (re)conceived as a choice providing a (usually small) likelihood of success and therefore (although this is often not the actual outcome) of improvement in the living conditions of the peasantry. It also involves some (probably larger) chance of failure, which would leave the individual less the costs of participation and might involve further costs in the form of state reprisals against a rebellious peasantry.

If an individual peasant's understanding of the existing social structural arrangement is placed in the losses frame, when confronted with a revolutionary choice, the decision to be made is between two losses: not to revolt entails the continued losses from the status quo, and to revolt entails an expected value of increased loss attendant to participation but also some smaller prospect for improvement. Here, framing predicts a greater predisposition toward risk (revolt) over the certainty of loss from the status quo in the attempt to return to the neutral reference point, that is, toward mitigating losses. Alternatively, if the current condition is not subjectively placed in the losses quadrant, then an individual will forgo the opportunity to improve his or her condition and opt for the status quo (i.e., reject revolutionary appeals).

FRAMING BY REVOLUTIONARY ORGANIZATIONS

If a losses frame is a first necessary condition for revolt, then the initial task for a revolutionary organization is to adopt—or construct—a worldview that effectively communicates to individual peasants the understanding that existing social-structural arrangements are worse than in some "normal" past.[16] It is interesting to note that this is nicely paralleled in a structural Marxian analysis. In classic Marxism economic terms, the argument is that technical improvements in the production process, given a labor theory of value, generate a series of crises leading to the monopolization of capital that in turn (and at the same time) increases the size of the proletariat and decreases the demand for labor. The result is depressed wages and the reality of not meeting subsistence needs (Loucks and Whitney 1983). Speaking more directly to peasants, Mao argued that "a man in China is usually subjected to three systems of authority" (political, clan, and religious) resulting in peasant oppression (Mao 1927, 44). Both these accounts describe the existing order as a process that operates systematically to the detriment of the individual. They also describe a process of systemic closure. There is no niche in the existing system where an individual can escape oppression.

However, there are also limits on the degree to which the ideology employed by revolutionary organizations can frame revolutionary choices; and these limits are the very reason why an analysis of social structure is necessary for an understanding of revolution. Ideological descriptions "assert the truth of relations between datable, localizable things in an objective world" (Ingram 1987, 23). In short, there is a point at which a value system must ultimately access aspects of empirical reality for its legitimization. If it does not, the contradictions between ideological assertions about the world and an individual's subjective assessment of his or her social context prevents its adoption. The successful framing of a revolutionary choice, then, is not separate from the existing structural context but is, in an important sense, related to it. This is a central point and suggests the degree to which the nature of the link between ideology and the empirical conditions I have described can inform our understanding about revolutionary processes.

In addition to the framing of choices, a revolutionary organization must overcome what William Hinton has noted in peasant communities as an "all-pervading individualism engendered by the endless, personal struggle to acquire a little land to beat out the other fellow in the market place. Peasants individually driven to bankruptcy viewed economic disaster not as a social but as a personal matter, to be solved in isolation by whatever means came to hand" (quoted in Popkin 1979, 251). To overcome fatalism, a sense of agency is necessary. Aside from fostering a predisposition towards risk, structural framing is central as it shifts the locus of failure from the individual to the existing set of social-structural relationships. Individualistic fatalism is replaced with the notion that change can be effected by individual action through organizational means. Recall that when faced with losses, in the attempt to return to a normal condition, individuals often embark upon risky ventures implying only a small probability of success. In other words, an individual's subjective assessment about the likelihood of successfully negating the losses inherent in the status quo through revolution need not be large to initiate action. This suggests that the hurdles to effective organization, while significant, are not as high as an assumption of macroeconomic rationality would posit, since certainty of a positive payoff is no longer a necessary condition for participation.

Recall the case of the lack of revolt in Oaxaca, which contradicts Skocpol's structural theory. This could be explained by the fact that individual perceptions by peasants of the stable economic structure in Oaxaca did not place them in the losses frame. As prospect theory suggests, they were predisposed to forgo the opportunity to improve their condition (meager as it was) through the risky venture of revolutionary action (afforded them by a collapse in effective sanctioning), instead choosing the certainty of the status quo. Alternatively, in revolutionary Morelos, peasants had become marginalized. Loss of land, income, and stability were all the result of the modernization drive. Here, the same revolutionary appeals confronted peasants with two choices both having an expected value of increased losses. A predisposition toward risk led peasants to choose revolt in the attempt to mitigate losses.

Next, recall Popkin's application of by-product theory in a description of peasant mobilization by the communists in Aman. While the provision of selective incentives (literacy campaigns and changes in the communal land system) could account for the decision to give support to communist organizations, it could not account for revolutionary action, which required a separate, additional calculation. Indeed, in Popkin's own historical account, such tactics were effective in attracting peasant support away from rival organizations only after the seizure of government by the communists. As Popkin puts it, "Seizing power, in other words, was but the beginning of the process of gaining control of the villages and of bringing about distribution in the countryside" (1979, 223).

Prior to the August Revolution in 1945, Popkin describes the condition of Communist party as being "at its lowest point" (1979, 218). Later, events in the Tonkin mountain region put individual peasants at extreme risk and reinvigorated the party. Popkin describes the period of the Japanese occupation, in which

> large tracts of land were forcibly diverted from rice production to Jute, and as large quantities of peanuts and rice were commandeered for the war effort, famine entered Tonkin. As early as 1943, with American planes bombing the rail lines to Saigon and preventing the shipment of food, there was starvation in the north. By 1944, there was mass starvation that finally took between five hundred thousand and two million lives in Tonkin. (pp. 219–20)

Now, revolutionary and nationalist appeals by the communists confronted peasants with two choices. The first, to reject revolutionary appeals and do nothing, entailed the continued loss of the status quo. The second, to join the communists and revolt, offered the risky proposition of action, which would entail increased risks and costs but also offered some possibility of a return to normalcy. When combined with a redoubled propaganda effort emphasizing nationalism and the removal of foreigners, Tonkin was perceived as a losses frame, and the attendant predisposition toward risk allowed the Communist party to garner enough support to carry off the August Revolution. It was largely Tonkin peasants who "emerged from the backlands and mountains to mount the August Revolution" (1979, 221).

CONCLUSIONS

Current theories of revolution, whether rational choice or structural, make at least one of two incorrect assumptions that inhibit their ability to resolve the agent–structure problem: (1) the role of structures is narrowly understood as simply confining or delineating the boundaries within which intentional action takes place; and (2) only a single model of individual action is assumed, and this model transcends structural context.

Structural accounts are necessary tools for understanding the susceptibility of regimes to revolution and defining the universe of conceivable postrevolutionary outcomes. Structural accounts of revolt speak best to potentials for action but cannot alone account for individual choices to revolt. More importantly, wedding a microeconomic decision rule to such structural accounts inadequately confronts the free-rider dilemma. As Coleman has recently stated, the structural benefits of successful revolutionary transformation are largely "independent of an individual's participation and provide him with no incentive to act. In fact, participation sometimes even leads to death. Yet in some revolts there is mass participation. How can this be? (1990, 495).[17]

Alternatively, sophisticated rational choice models, like Popkin's, have borrowed from the literature on collective action in an attempt to account directly for revolutionary choices. Here, the description of how and why peasant organizations gain support and prosper is enlightening. However, without addressing explicitly the social-structural context in which *revolutionary* action is nested, the logic of selective incentives breaks down, leading one to conclude that the choice to revolt is distinct from the decision to participate in a peasant organization as such.

Indeed, the weight of my discussion suggests that in certain circumstances, Coleman's rhetorical question needs to be reformulated. Rather than the aggregation of individualist rational calculations, collective rebellion should be conceived as an interaction between structure and intentional action—grounded in an empirically observed predisposition toward risk in the face of a choice between losses. Therefore, beyond a structuring of incentives, social structure can lead individuals to evoke decision rules *alternative* to microeconomic rationality. Collective action is then the aggregation of individualistic decisions to mitigate losses in certain structural contexts. Of course, in a comprehensive explanation of social revolution, an agent–structure theory of revolutionary choice is compatible with both methodological structuralism and rational choice models. Both structural potentials (regime susceptibility) and effective organizations are themselves necessary conditions for social revolution. Agent–structure theorizing is thus a necessary supplement to extant approaches attempting to explain social action.

The weakness of this conceptualization, however, is that it only partially resolves the agent–structure problem. In this description of revolt, risky social structures are assumed a priori—their genesis is not accounted for. The strength is in the attempt to address agent–structure questions (which of course extend beyond revolution) explicitly and thus avoid the pitfalls of rational or structural reductionism.

Finally, in this framework, the role of revolutionary ideology is central and needs to be reintegrated into a model of peasant revolt. Both Skocpol and Popkin explicitly deny its importance. Simon notes that in order to deduce the boundedly rational choice in a given context, "we must know the choosing organism's goals, *the information and conceptualization it has of the situation*, and its abilities to draw inferences from the information it processes" (1985, 294; emphasis mine). It is here than an examination of ideological premises as "framers" would be useful. Knowledge about the operation of ideas in local contexts, grounded in the local experience of individuals, is, then, a necessary component for an understanding of peasant revolt and one that calls for closer examination.

Notes

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1. Other examples here include Farhi's (1988) structural analysis (building explicitly on Skocpol's work) on Iran and Nicaragua. Alternatively, Muller and Seligson (1987) employ exclusively structural variables in a quantitative cross-national study of insurgency. The recent work on rational choice explanations of revolt includes Taylor's (1988) edited volume; see also Muller and Opp (1986). For compelling general arguments in favor of methodological individualism and structuralism see Coleman's *Foundations of Social Theory* (1990) and Tilly's *Big Structures, Large Processes, Huge Comparisons*, respectively (1984).

2. Because the argument here is largely theoretical, the empirical cases discussed are intended to highlight central theoretic points; they are not meant to be sufficient evidence for a "test" in themselves. The application of cases here is informed by Eckstien's (1975) discussion of the disciplined idiographic use of case studies.

3. Structural and individualistic theories of revolution have been discussed together in the literature reviews of specific revolutions, but these lack any integrative attempt. See, e.g., Akhavi 1989 on Iran; Chilcote 1989 on Portugal; and Duran 1982 on Mexico. Hawes's (1990) synthetic attempt, while it ignores the agent–structure problem, is a good summary of contending approaches.

4. Skocpol defends the ability of structural methodologies to generate discriminating tests (1979, chap. 1, esp. 33–40).

5. Note that Meadwell (1988) criticizes this structural hypothesis on its own terms by arguing that the relationship between autonomy and revolt is spurious and that solidarity is theoretically neither a necessary, nor a sufficient, condition for revolt.

6. For a more complete discussion of the implicit, but inescapable, relationship between structural determinism and microeconomic models of choice see Barry Hindess's *Political Choice and Social Structure* (1989).

7. Nor is it evident, e.g., that moral outrage or a decline in regime legitimacy necessarily leads to revolutionary choices. This is essentially the same criticism leveled at the earlier "relative deprivation" hypotheses of Davies (1962) and Gurr (1970). Specifically, there is no necessary reason to expect that the frustration one experiences, if translated into aggressive action, will be directed at overthrowing the status quo, rather than being vented elsewhere. Thus, moral outrage, or a decay of legitimacy, alone cannot account for rebellion against the state.

8. Taylor is not entirely antithetical to such an accounting despite his explicit defence of the analytic utility of microeconomic rationality.

9. Olson's description of the American Medical Association provides an exemplary case. A collective good provided by the AMA is influence (lobbying) in the formulation of tax policy and health legislation. Because any member of the medical community can receive such benefits, there is no incentive to contribute to the AMA. However, the AMA also provides to its members such noncollective, selective benefits as information on tax loopholes, new medicines, and support

in malpractice litigation, which only members who contribute can receive. Individual contributions are in order to receive these selective benefits, and any surplus resources collected by the AMA are then devoted in part to the lobbying effort. In this sense, lobbying is a by-product of rational choices by individuals who wish to garner other selective incentives.

10. Often, otherwise solid theoretical work on rational choice models of collective action fails to address the social-structural context in which such action is nested (see, e.g., Oliver 1980; Oliver and Marwell 1988; and Oliver, Marwell and Teixeira 1985).

11. For an extended discussion of "bounded rationality" and its role in political science see Simon 1985.

12. The only other difference is that this curve is steeper than in the gains quadrant, representing individuals' heightened sensitivity to losses.

13. In other words, experiments in the framing literature suggest that rather than choosing a certain smaller loss, individuals attempt to mitigate all loss and return to the status quo by choosing a risky option even though it has only a small likelihood of success. This is the case even if the gamble has a higher expected negative value than the certain loss.

14. As an example, Kahneman and Tversky (1982) have described the essential characteristics of framing through an illustration.

> *Gains Frame.* Imagine a choice between two options that imply personal gains: (1) a sure gain of $80; (2) a risky venture with an 85% chance of winning $100 and a 15% chance of winning nothing. When confronted with this choice, most prefer the sure gain over the risky venture even though the expected monetary outcome is $5 less.
>
> *Losses Frame.* Now imagine a second set of options that imply personal losses: (1) a sure loss of $80; (2) an 85% chance of loosing $100.00 and a 15% chance of loosing nothing. Here, individuals prefer the gamble over the sure thing even though the expected monetary loss is $5 greater.

Aside from the fact that in both scenarios individuals deviate from the predictions of simple microeconomic utility maximization, the central point here is, "Preferences between gains are risk averse and preferences between losses are risk seeking" (ibid., 160).

15. Here, in the attempt to infer a losses social frame, a structural or class-based analysis is quite useful. For analytic purposes, then, a losses frame can be inferred empirically. The intent is to follow Snidal's (1985) suggestion that incentive structure (here a losses social frame) be accessed *independently* of the observed choices of individuals, thus avoiding the tautology trap that captures some rational choice explanations.

16. This point should not be interpreted as an assertion that revolutionary movements are founded on the notion of "returning to the status quo." The notion of framing is not inconsistent with the understanding that revolutionary organizations most often aspire to move beyond normalcy, to an improved condition.

17. Coleman's answer is akin to Taylor's in that he relies upon community (or group) norms to provide effective incentives to participate (see Coleman 1990, 495–96).

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POLITICS, INSTITUTIONS, AND WELFARE SPENDING IN INDUSTRIALIZED DEMOCRACIES, 1960–82

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We examine the roles of democratic politics and political institutions in shaping social welfare spending in 18 contemporary capitalist democracies. We explore the social spending consequences of government partisanship, electoral competition and turnout, and the self-interested behaviors of politicians and bureaucrats, as well as such relatively durable facets of political institutions as neocorporatism, state centralization, and traditionalist policy legacies. Pooled time series analyses of welfare effort in 18 nations during the 1960–82 period show that electoral turnout, as well as left and center governments increase welfare effort; that the welfare efforts of governments led by particular types of parties show significant differences and vary notably with the strength of oppositional (and junior coalitional) parties; and that relatively neocorporatist, centralized, and traditionalistic polities are high on welfare effort. Overall, our findings suggest that contrary to many claims, both partisan and nonpartisan facets of democratic politics and political institutions shape contemporary social welfare effort.

To date, no consensus has been reached on the controversy launched by Cameron (1978), Castles and McKinlay (1979), and Stephens (1979) over how politics matters for welfare spending in industrialized democracies. Despite much quantitative empirical study, research has not determined to what extent, if any, welfare policies are shaped by government partisanship, the self-interests of government personnel, mass electoral preferences, or neocorporatist institutions.[1] Dissensus over these recurrent questions persists in large part because theoretical frameworks and empirical models of individual studies have been too narrow. Typically, quantitative studies have only focused on one or another subset of already controversial explanatory factors (e.g., left party strength), thereby courting biased findings by omitting variables that may be correlated with included variables (Johnston 1984, 259–65). Moreover, quantitative studies have frequently risked unstable findings by using coarse measures and minute samples—frequently with as few as 15 cases. In short, unreliability, as well as bias, has impaired the progressive cumulation of research findings.

Not only have individual studies been narrow. As we shall illustrate, the range of explanatory factors explored in the whole quantitative literature has been limited. This narrowness has exacerbated discensus over the political causes of welfare spending by excluding quantitative analyses of political forces often highlighted in historical and theoretical works—in particular, political institutions and democratic oppositions. For example, as regards the historical salience of political institutions, Germany's 1880s inaugurations of national social security programs; turn-of-the-century Scandinavian innovations in universalistic welfare legislation; and Britain's postwar consolidations of universalistic, flat-rate, uniform payment social security systems all arose within highly centralized states (Baldwin 1990).[2] Moreover, theoretical reasoning about the relevance of political institutions to welfare spending has expanded significantly since certain seminal works (Dahl 1982, chap. 4; Evans, Reuschemeyer, and Skocpol 1985; Lijphart 1984; and Powell 1982). Yet consideration of democratic political institutions has been scarce in the burgeoning quantitative–empirical literature on welfare spending (but see Cameron 1978). Similarly, while strategic interactions of partisan governments with their oppositions figure prominently in both political history and theory (Baldwin 1990; Downs 1957), influences of democratic oppositions upon government welfare legislation have been neglected in quantitative studies of welfare spending.

We attempt to remedy these problems by simultaneously assessing social welfare impacts of government partisanship, electoral competition and turnout, bureau voting of public sector interest groups, and reelection-oriented behavior of incumbent politicians. More novelly, we test for policy influences of democratic oppositions (and "junior partners") and greatly extend consideration of democratic political institutions. We use 23-year times series for 18 nations, significantly expanding the data base of conventional studies; and we assess social welfare impacts of democratic politics and political institutions in the context of models that account for impacts of welfare needs, economic capabilities, and economic structure.

To anticipate our major conclusions, high voter turnout, as well as center and left government leadership, augments welfare effort. Center-led governments do so even more than Left-led ones. The strength and partisanship of democratic oppositions matter for effects of partisan governments. Social welfare effort is enhanced by neocorporatist institu-

TABLE 1

Political Influences on Social Welfare Effort in Capitalist Democracies across Three Theoretical Perspectives

TYPE OF FACTOR	PERSPECTIVE		
	SOCIAL DEMOCRATIC CORPORATIST	POLITICAL DEMO-CRATIC	STATIST
Political agency	left and center party strength	voter turnout; electoral competition	state-sector employment; electoral cycles
Political institutions	working class organization and political incorporation	majoritarian and consensus aspects of political systems	political/ administrative capacity; conservative policy legacies

tions, state centralization, and traditionalistic policy legacies.

THEORY

Social Democratic corporatist, political democratic, and statist theoretical perspectives on the politics of welfare effort help structure our inquiry. Drawing on both neopluralist and neo-Marxist theories, the social democratic corporatist perspective emphasizes the importance of class-linked parties and interest organizations for welfare policy determination (e.g., Korpi 1983; Stephens 1979). The *political democratic* tradition of scholarship stresses the welfare policy impacts of such nonpartisan facets of electoral politics as electoral competition and turnout (Dye 1979; Pampel and Williamson, 1989). The *statist* perspective highlights the policymaking behavior of incumbent politicians and bureaucrats (Mueller 1979, chap. 6) while placing a special stress on state institutions (Skocpol 1985).

Upon close inspection, each of these perspectives can be said to have an explicitly *agency-centered* facet stressing the political action of voters, groups, parties, political executives, and administrators. Each perspective also has a *political-institutional* facet that highlights structural features of the state resurgent in recent "state-centered" accounts of politics, as well as many other aspects of political institutions (see also Alford and Friedland 1985). The individual political agency and political-institutional components of each perspective are summarized in Table 1.[3]

Political Agency

Social democratic corporatist theory stresses the mutually reinforcing roles of strong left (i.e., Socialist, Labor, and Social Democratic) parties and strong Labor movements. This is the case both for works of Marxian origins, in which government control by left parties and the existence of strong unions are effective, albeit reformist, manifestations of working-class mobilization (Korpi 1983; Stephens 1979) and for neopluralist studies, in which these parties and unions represent consequential political forces in democratic societies (Cameron 1978; Hibbs 1987). Moreover, some scholars working in this tradition have argued that governments led by centrist secular and Christian Democratic parties have advanced income-security policy (e.g., Castles 1982; Wilensky 1981).[4] From the social democratic corporatist perspective, the most central proposition to emerge has been that left and center (non-right) party leadership of government generates higher welfare effort than right and "indeterminate" party leadership. However, some have suggested important roles for oppositional, as well as governing, parties.

The growth of electorally consequential left parties, or "contagion from the Left," has sometimes led conservative governments to welfare-expanding reforms (Esping-Andersen 1990; Heclo 1974). Similarly, welfare policies of Left-led governments may be moderated by strong center and right oppositions (Heclo and Madsen 1987; Przeworski 1985; Stephens 1979). Center parties may vie with left parties for "median voters" and right parties may compete with them for "Tory" workers. Furthermore, the moderation of government welfare policies by democratic oppositions may be supplemented by the presence of junior governing partners. In particular, both Left- and Right-led governments often rely on the formal and informal support of minor centrist parties. When they do, these strategically positioned actors may moderate—indeed, redirect—the welfare orientations of dominant governing parties (Baldwin 1990, chaps. 3, 5).

A few scholars have qualitatively analyzed political oppositions (Baldwin 1990; Heclo 1974). A few more, using large-sample multivariate methods, have at least skirted the topic by examining the electoral support of parties, albeit without regard for their governing or nongoverning statuses (e.g., Korpi 1989). However, no systematic, multinational evidence exists on the consequences of democratic oppositions for governmental welfare policies. Here, we fill this vacuum by hypothesizing that Right-led governments are pressured in a prowelfare direction by electorally consequential left and center oppositions (and junior governing parties). In addition, we hypothesize that Left-led governments are pressured to moderate prowelfare policies by oppositions (and junior governing parties) of the Center and Right. Finally, extending the logic of our argument to all major party types, we conjecture that prowelfare policies of Center-led governments will be intensified by strong left oppositions and moderated by strong right oppositions.

Scholars within the social democratic tradition have downplayed effects of partisanship and stressed the social welfare impacts of such nonpartisan facets of electoral systems as electoral participation and competition (Dye 1979; Pampel and Williamson 1989). Specifically, they suggest that parties in highly competitive electoral environments will make prowelfare appeals (e.g., promises of more income security) to "median" voters in hopes of gaining electoral majorities (see also Key 1949). They also argue that increases in voter turnout pressure governmental incumbents to spend more on welfare because rises in turnout reflect the entry into politics of new, lower-status voters. Recently, they have stressed the acts (or agency) of individual "median" voters and other voters underlying such "systemic" effects of elections. Overall, this perspective suggests that higher levels of electoral competition and of voter turnout lead to greater welfare effort.

State institutions aside, statists have stressed policy consequences of the self-interested behavior of bureaucrats and (incumbent) politicians. With respect to bureaucrats, Niskanen's (1971) assumption that budget-maximizing state bureaucrats act collectively to maximize public spending has been supported by studies of the American states and the industrialized democracies (Garand 1988, 840; Korpi 1989). These considerations suggest that the relative numbers of state-sector employees in a population promotes welfare effort. With regard to politicians, the assumption of vote maximization has grounded the proposition that incumbent politicians seek to expand social spending during the year or two preceding elections (e.g., Tufte 1978). Although more often challenged than supported by empirical evidence, this proposition nonetheless continues to receive frequent attention (e.g., Keech and Pak 1989). Thus, we explore the hypothesis that preelection welfare effort tends to exceed postelection effort.

Political Institutions

Consideration of the statist perspective naturally directs attention to the policy significance of political institutions. After reviewing the treatment of institutions in each perspective, we empirically identify some dimensions of democratic political institutions and pose hypotheses about their consequences for welfare policy. *Political institutions* is broadly contrued to connote a very wide range of elements that extends beyond the formal organizations of the state to encompass political parties and interest associations and, among such associations, even principally economic organizations (e.g., unions) that serve important interest group functions.

Traditions of union and left party strength ground social democratic corporatist politics. By *union strength* we mean a convergence of high levels of union density and organizational cohesiveness (see Alvarez, Garrett, and Lange 1991; Korpi 1983); by *left party strength* we mean a historical pattern of frequent left party governance (e.g., strong cabinet participation) and extensive left party voter mobilization (Korpi 1983; Powell 1982). *Social democratic corporatist politics* connotes a political context conducive to policies that concurrently pursue job and income security and aggregate economic growth (Alvarez, Garrett, and Lange 1991; Hicks 1988a, 1988b). Traditions of strong unions and left parties also connote high probabilities of working-class incorporation into institutions of tripartite (business–labor–state) social and economic policy formation; that is, they connote working-class interest representation, as well as working class political mobilization (Przeworski 1985).

Esping-Andersen's (1990) discussion of state institutions goes beyond traditional social democratic concerns to argue that the policy legacies of the absolutist state and of associated resistance to mass enfranchisement are the sources of a (traditional) conservative configuration of policy traits and goals present in some contemporary welfare states. This configuration, termed a *conservative policy regime*, is marked by vestiges of noblesse oblige, preemptions of progressive reforms, and welfare programs that reinforce the status prerogatives of public (e.g., civil servant) and private (e.g., ethnic) groups, thereby fostering clientelism. Furthermore, we suspect (following leads in Lipset 1983) that more general historical legacies of class and status rigidities may have similar consequences for contemporary welfare states. Explicit attention to such hierarchical or traditionalist legacies may be especially important for the German, Austrian, and Japanese cases, given the Allied decentralization of these nations' state structures following World War II (see Katzenstein 1985; Wolferen 1989). Postwar Allied institutional reforms may have obscured traditionalist legacies (e.g., democratic reforms may have obscured legacies of administrative paternalism) in these nations. As Esping-Andersen (1990) suggests, such conservative policy regimes, while distinct from social democratic corporatist ones, are also likely to lead to greater social welfare effort than liberal, market-oriented regimes.

Lijphart's *Democracies* (1984) impressively maps variations in formal political institutions across stable capitalist democracies. Lijphart organizes his survey of democratic institutions around majoritarian and consensus modes of democratic politics. His term *majoritarian* refers primarily to government by "minimal winning coalitions" and to institutions tending to support such government; that is, it refers to the rule of slim majorities and to the kinds of complementary institutions best illustrated in the United Kingdom and New Zealand: one-party and bare majority cabinets, fusion of governmental branches and jurisdictional centralization, unicameralism, two-party systems, single-cleavage patterns of conflict, and flexible (even unwritten) constitutions (chap. 1). Consensus government refers to rule by more diverse, contingently negotiated coalitions and to governments much more constrained by consensus than are majoritarian ones. More specifically, it refers to Swiss-like, antimajoritarian factors that require or encourage the sharing of power between the majority and the minority (grand coalitions), the dispersal of power (among executive, two legislative chambers, and minority parties), a fair distribution of power (proportional representation), the delegation of power (to territorially or nonterritorially organized groups), and a formal limit on power by means of the minority vote (p. 30).

Operationally, Lijphart's effort culminates in a two-dimensional factor-analytical model of the majoritarian/consensus character of political institutions. The first factor connotes a majoritarian *minimal winning coalition* dimension that emphasizes the fewness of "effective parties" and issue cleavages, disproportional representation, and executive and minimal majority dominance of government (1984, 214). The second dimension—most closely associated with unicameralism, revenue centralization, and constitutional flexibility—is singled out by Lijphart as *federalism* (p. 215). However, care is needed here: as the scale scores federal nations low, a unitary characterization is more appropriate. We shall refer to this second dimension as Lijphart's majoritarian unitary dimension.

The welfare implications of the majoritarian/consensus dimension for welfare effort are indeterminate. On the one hand, majoritarian extremes may notably facilitate partisan enactment of distinctive policy goals (see Jackman 1986). On the other, their characteristically two-party forms promote "Downsian" platform convergence (Lijphart 1984, chaps. 1–2). Moreover, *consensus* systems may arise in political contexts that legitimize—indeed, foster—redistributive policies (Lijphart and Crepaz 1991; Masters and Robertson 1988; Powell 1982, chap. 9). Rather than offer specific hypotheses now, we shall return to this issue after our empirical assessment of dimensions of democratic political institutions.

For statist students of the welfare state, *institutions* connotes "democratic institutions," as well as policy structures, precedents, and experiences that enhance nations' policymaking capabilities—particularly those that foster effective policy ideas, cohesive legislation, and effective implementation (e.g., Skocpol 1985; Weir, Orloff, and Skocpol 1989). Reduction of all such capacities to a few measurable dimensions is infeasible, yet some dimensions stand out. One is state *centralization* in the sense of jurisdictional and administrative consolidation of policymaking and implementing authority (Orloff and Skocpol 1984). A second such dimension is *bureaucratization* itself, although the critical threshold of bureaucratization relevant to welfare effort may have been crossed in our 18 democracies by the 1960s (Weir, Orloff, and Skocpol 1989). An additional factor entails a nation's *policy precedents* and experiences. For example, a nation with a broader scope and a longer experience of major social programs will tend to have stronger grounds (precedents, advocates, etc.) for extending welfare policy (Wilensky 1975; Skocpol 1985).

Fortunately, state centralization, bureaucratization, and program precedents are all measurable (Pampel and Williamson 1989). We now analyze measures of these and other institutional variables in an effort to map some principal dimensions of cross-national variability in political institutions empirically. In order to identify some principal dimensions of political institutions, we factor-analyzed a number of temporally invariant institutional variables for the 18 continuously democratic industrialized democracies noted in Table 2. First, we sought to identify and index a summary dimension or two of state policymaking and administrative capabilities by tapping measures of state centralization, bureaucratization, and welfare program experience and Lijphart's majoritarian/consensus dimensions. We also attempted to identify a dimension of long-term working-class mobilization by means of measures of union density and cohesion, the historical incidence of left party government, and the extent of working-class electoral mobilization. Finally, we sought evidence about the clustering of traditionalist policy legacies with other facets of state institutions. (Our index of legacies combines scales of *legacies of absolutism* and *resistance to democracy* adapted from Esping-Andersen 1990, and a scale of *class rigidity* adapted from Lipset 1983).[5]

These measures of political institutions were analyzed by means of a principal components factor analysis of the 18 nations. After orthogonal rotation, this yielded the (structure) matrix of factor loadings displayed in Table 2. Of three sharply defined and distinguishable dimensions, one is a readily interpretable *left corporatism* dimension with strong loadings for the four social democratic corporatist items and loosely negligible loadings for all other items. A second *state centralization* dimension emerged with strong loadings for all administrative items except bureaucratization (.189) and, arguably, the *unitary/federal* dummy (.570). Interestingly, this latter item loads notably (.486) on the third, *bureaucratic traditionalism* dimension, which has strong loadings for traditionalist legacies (.802) and bureaucratization (.829). As Table 2 reveals, this coalescence of bureaucratization with traditionalism typifies non-Protestant nations (Austria, Belgium, France, Ireland, Italy, and Japan) known for their highly permeable, clientelistic administrations and is epitomized by the nationally centralized, but locally quasi-religious, quasi-private administrative apparatus of the Netherlands (Lijphart 1977). It reaches its nadir with the United Kingdom and its settler colonies. Only Denmark—a late monarchy, but Protestant and not renowned for its clientelism—mars the pattern.

Overall, we expect nations that rank relatively highly on each of these dimensions to have relatively high levels of welfare effort. We expect "left corporatism to affect welfare effort positively because it denotes the facets of working-class political economic mobilization and incorporation most likely to intensify pressures for more welfare effort. We expect state centralization to augment welfare spending because centralized/unitary government reduces veto points for the obstruction of prowelfare reforms and streamlines political instruments for the enactment of such reforms (see Cameron 1978). On the assumption that national political organizations can exert more prowelfare pressure on national policy than dispersed actors in subnational arenas, these aspects of centralization should augment welfare effort. Moreover, the

TABLE 2

Dimensions of Political Institutions: Orthogonally Rotated Principal Components circa. 1960

ITEMS	FACTOR 1: "LEFT CORPORATISM"	FACTOR 2: "STATE CENTRALIZATION"	FACTOR 3: "BUREAUCRATIC PATRIMONIALISM"
Variables			
Union strength	.897	.005	.200
Union centralizaton	.819	.041	.350
Class mobilization	.765	.318	−.003
Left rule, 1946–60	.906	.342	−.117
Revenue centralization	−.065	.880	.185
Unitary/federal	.299	.570	.486
State centralization	.329	.828	−.031
Early program consolidation	.144	.642	.590
Majoritarian—unitary	.224	.759	−.422
Majoritarian—minimal winning coalition	.270	.085	−.484
Welfare bureaucratization	.222	.189	.829
Traditionalist legacies	.141	−.018	.802
Nations			
Australia	−.335	.027	−1.259
Austria	.994	−.802	.855
Belgium	.581	.515	.582
Canada	−.731	−1.191	−1.326
Denmark	.885	.139	.955
Finland	1.088	−.274	−.234
France	−.293	.808	.414
West Germany	−.566	−1.176	.687
Ireland	−1.338	1.306	.599
Italy	−1.016	.801	1.142
Japan	−.846	−.608	.589
Netherlands	.030	.474	1.458
New Zealand	−.113	1.236	−2.361
Norway	1.803	−.147	−.136
Sweden	1.983	−.063	−.667
Switzerland	−.591	−1.626	−.038
United Kingdom	−.325	1.908	−.553
United States	−1.210	−1.328	−.706

Note: Three factors, specifically three orthogonal, varimax-rotated principal components, specify the dimensions. The factors are described by two matrices, one of variable loadings per nation, a second of factor scores per variable (or factor item). See Appendix on measurement of items in these factors.

relatively long-standing consolidation of major programs associated with centralization connotes statutory and administrative bases for the expansion of social welfare budgets. Finally, we expect bureaucratic traditionalism to affect welfare spending positively because it denotes tendencies toward noblesse oblige, patronage politics, and high-status bureaucrats (with specialized pension programs and the like). As noted, these tendencies should be more conducive to welfare spending than free market liberalism (imagine Italy versus Canada), yet they might congenially complement socialist politics (as in postwar Austria).

Welfare Needs, Economic Capacity, Economic Structure

With regard to environmental influences on welfare spending, each of our perspectives acknowledges the welfare impacts of economic needs, resources, and structures. Because the operation of these forces, although political, falls short of the manifestly political ones that we are highlighting in this paper, we treat them as supplementary explanatory factors and statistical controls.

First, rates of old age, unemployment, and other forms of need (or "dependence") that denote program eligibility are typically regarded as major sources of automatic (and occasionally discretionary) variations in social spending. In addition, studies indicate that inflation, by pressuring the legislation of upward adjustments of benefit levels, triggering in-place cost-of-living allowances, and augmenting revenue availability via bracket creep (all in excess of increases in real income), may augment welfare effort (e.g., Pampel and Williamson 1989; see also our earlier work, Hicks, Swank, and Ambuhl 1989). Moreover, numerous authors have hypothesized that economic development devolves new welfare functions on to the nation-state while concurrently generating the collective attitudes (altruism) and resources (tax bases) that facilitate "welfare expansion" (Wilensky 1975). Thus, we expect that the relative size of the aged population, the unemployment rate, price levels, and economic development will all increase social welfare spending.

Students of the welfare state often underscore the welfare impacts of international economic structure, particularly openness to the international economy. Such openness is a source of direct pressure on welfare effort, as well as of neocorporatist arrangements that promote it (Cameron 1978; Katzenstein 1985). The mechanisms grounding direct effects of openness consist of pressures for income security outlays (e.g., social wages and subsidies) that buffer individuals and firms from international volatility. Accordingly, we expect that the greater economic openness, the higher welfare spending.

Finally, the 1973 OPEC oil shock is widely regarded as a watershed between two macroeconomic eras (Hicks, Swank, and Ambuhl 1989; Swank 1988). The post-1973 unraveling of the Keynesian reconciliation of interventionist government and economic growth, as well as the post-1973 ascendance of antistatist movements and governments, arguably restrained welfare spending (Hicks, Swank, and Ambuhl 1989). However, stagflation, by extending the duration and experience of unemployment (and underemployment) increased the need and demand for welfare effort beyond what can be captured by our standard measure of the unemployment rate (Roubini and Sachs 1989). Hence, we control for a possible post-1973 shift in welfare effort without predicting its direction.

EMPIRICAL MEASURES, METHODS, AND ANALYSES

Measurement and Operationalization

The present work analyzes welfare effort in the 18 largest, most affluent and continuously democratic post–World War II nations (see Table 2). It does so for 1960–82, years for which complete and comparable data are available. Operationalized variables are detailed in the Appendix. However, a few immediate comments may help to clarify our measures.

The welfare effort ratio of welfare spending to gross domestic product is computed with the International Labor Organization (ILO) expenditure data and gross domestic product resource data. The gross domestic product's lesser vulnerability to distorting trade and exchange rate fluctuations makes it preferrable to gross national product data. Total expenditure data on social welfare spending from the ILO's *Cost of Social Security* improve on the comprehensiveness and comparability of analogous Organization for Economic Cooperation and Development (OECD) data by including health spending and some in-kind benefits. Welfare spending is defined here, (as by the ILO) as governmental spending relating to schemes, transfers, and services that (1) grant curative or preventive medical care, maintain income in case of involuntary diminuation of earnings, or grant supplementary income to persons with family responsibilities; (2) are legislatively sanctioned; and (3) are publicly or quasi-publicly administered (see ILO 1988, 1). A focus on total spending is chosen, first, so that we might maintain continuity with a large literature stressing such aggregates (see Castles 1982; Hicks, Swank, and Ambuhl 1989; Pampel and Williamson 1989; Swank 1988). A summary focus is used, second, because top-down theorists stress the causal priority of large aggregates over their components—which are decided on as shares of more-encompassing aggregates (Kamlet and Fischer 1984). Also, expenditures on subcategories of social welfare policy may function as alternate, functionally equivalent means to collective welfare. Finally, the analysis of aggregate spending is parsimonious, as well as important, while that of specific programs, although imperative, must lie beyond the present, already complex, effort.

To avoid simultaneity bias and curtail "specification searching" for optimal lags, explanatory variables were generally lagged one year except where theory or substance demanded otherwise. One occurred for age, which was measured so as to adjust empirically to cross-national differences in early retirement and pensioning practices and thus to minimize any underestimation of "automatic" age effects.[6] A second exception, the unemployment rate, is specified as a two-year moving average of unemployment at t and $t - 1$ because unemployment clearly has prompt "automatic" effects via unemployment compensation spending yet may elicit more discretionary policymaker responses after a year or so. Election periods, which may be targets for enhanced program benefits at t, as well as for economic stimulation by means of spending increases a year before, are for t and $t + 1$.[7]

The party strength measures reflect the potentially ample range in governments' lags in responding to environmental stimuli.[8] To take account of this, both governmental and oppositional party strength measures were cumulated over the range of years that proved consequential in exploratory distributed lag analyses of effects of partisan measures on welfare effort for years $t - 6$ through t.[9] Specifically, party strength measures were cumulated (i.e., averaged) for lags of one through four years ($t - 1$ through $t - 4$).

Turning to yearly operational definitions of party strength, parties were designated as left, center, or right using Castles and Mair's (1984) typology. More originally, strength itself was measured in terms of (1) party leadership of government and (2) party electoral and legislative strength in opposition (see Appendix). For each type of party, government leadership was measured as the proportion of years $t - 4$ through $t - 1$ during which a party led government in the sense of having a decisive majority of its cabinet positions. A type of party was judged to lead a government if it had at least 120% of the number of cabinet portfolios of each other type; the present criterion yields a reasonable 26 (out of 414) nation-years of "indeterminate" government.[10]

With governmental leadership measured, we can turn to measuring governmental opposition and junior partnership. This combination of party statuses is measured as the four-year average (years $t - 1$ through $t - 4$) of two annual measures of the strength of parties *not* leading governments: the year's average of the party type's proportion of legislative seats and proportion of the popular vote in the last major election when the given party type did not govern.[11] Our measurement of opposition allows for effects of nongoverning parties (i.e., ones not leading governments) upon *effects* of governing ones to be gauged directly by means of the addition of a set of opposition–government product, or interaction, terms. For example, we operationalize classical "left contagion" as the interaction of left opposition and right governmental leadership by means of a term defined as the product of these two variables (i.e., left opposition × right governmental leadership, or *left–right*). The slope estimate for this term is interpretable as the effect of left party oppositional strength upon the (Left-contingent) effects of right party government upon welfare effort. Here, symmetry and thoroughness mandate three pairs of opposition–government interactions implying that left government effects on social welfare effort vary inversely with the strength of center and right oppositions, that right government effects vary directly with (i.e., are pressured in a positive direction by) the strength of center and left oppositions, and that center government effects vary directly with left opposition and inversely with right opposition.

Note that one of the four government leadership measures (left, center, right, or indeterminate) must be excluded from our regressions. More specifically, because government leadership measures are averaged from a mutually inclusive and exhaustive set of categories (i.e., left, center, right, indeterminate) whose scores sum to a constant 1.0 (across categories for each nation-year), estimates of partisan government effects are contrasts from a benchmark category. Moreover, in order to avert perfect multicollinearity among regressors, estimation of these contrasts requires that we omit one category of government leadership from our equations. We select the indeterminate category of government leadership as our benchmark and omitted category for two reasons. First, we expect left and center government effects on welfare effort similarly to exceed right and indeterminate ones, so that either the right or indeterminate category might serve. Second, the indeterminate category, unlike the right one, does not figure in our interaction hypotheses and, as a result, does not require specification among our regressors. Thus, omission of the indeterminate category is preferred to omission of the right one.[12]

Note that all three measures of opposition strength may be included together in our regressions. These measures are not constrained to sum to any particular total. Thus, values of one category are not fixed by values of the other two: degrees of freedom are sufficient for estimation of all three.

Specification

The general specification of our baseline model (or noninteractive) model, including signs for hypotheses, is as follows:

$$\begin{aligned}
\text{Welfare effort} = {} & a + B(1)\text{turnout} + B(2)\text{competition}\\
& + B(3)\text{state employee} + B(4)\text{election period}\\
& + B(5)\text{left gov't} + B(6)\text{center gov't}\\
& +/- \; B(7)\text{right gov't} + B(8)\text{ left opposition}\\
& + B(9)\text{center opposition}\\
& +/- \; B(10)\text{ right opposition} + B(11)\text{left corporatism}\\
& + B(12)\text{state centralization}\\
& + B(13)\text{bureaucratic traditionalism}\\
& + B(14)\text{Ln gross domestic product}\\
& + B(15)\text{price levels} + B(16)\text{unemployment rate}\\
& + B(17)\text{aged} + B(18)\text{openness}\\
& +/- \; B(19)\text{post-OPEC} \qquad \textbf{(1)}
\end{aligned}$$

We have no favored hypotheses for right governments or oppositions.

Our opposition–government interaction model is more elaborate. To Equation 1 it adds product or interaction terms for pairs of interacting variables. Interpretations of estimates for them, as well as for variables multiplied to construct them, are complicated. In particular, estimates that come attached to government and government–opposition terms now serve to describe equations expressing *variable* welfare effects of partisan government strength as functions of oppositional strength. Most crucially, estimates for interaction terms are now slopes linking variable effects of governments led by particular parties in these functions to levels of oppositional strength for particular parties. Government estimates are now intercepts in these functions, *not* the so-called "main" effect that might be suggested by an analogy to interactions in analysis of variance tables.[13]

Estimation

The pooling of time series ($T = 23$ years) and cross-sections ($N = 18$ nations) permits large-sample analyses that draw on temporal and cross-national variation. However, equation errors will typically be heteroscedastic and autocorrelated within units, as well as autocorrelated across units, thereby degrading estimator precision (Kmenta 1988, chap. 12; Stimson 1985). To correct for these maladies, analyses are performed using Park's generalized least squares model with corrections for (AR1) autoregressive, heteroscedastic, and contemporaneously/cross-sectionally correlated errors (see Kmenta 1988). Intercepts may also differ across units, yielding biased estimates. However, addition of the three political-institutional dimensions to this model should amply adjust for strictly cross-national differences in welfare effort, since these dimensions alone explain 60% of such variance. Finally, with regard to R-squareds, those in the tables are Buse R-squareds, which approximate the real R-squared conception of squared correlations between observed and predicted values of untransformed *Y*-values (White 1988). The Durbin–Watson *d*s are adjusted for gaps in our data—in this case for transitions between successive, "stacked" national times series.

Analytically, we first estimate a basic additive model and then estimate one that includes opposition–government interactions.[14] As a check on robustness, we next apply Tukey's (1958) "jackknife" to a key equation. The jackknife procedure tabulates destabilizations of estimates by excessively influential cases and provides alternative coefficient estimates that are robust in the face of such cases. To highlight robust findings, we reestimate the key equation after deletion of variables revealed unstable by jackknifing.

FINDINGS

Results support expectations, strongly rejecting most null hypotheses. Equations fit the data well, explaining over 90% of the variance in welfare effort; and they yield satisfactory Durbin–Watson *d*s (around 1.90).

In the additive equation of Table 3, column 1, nearly all hypotheses are supported at the .05 test level. Exceptions are provided by the unexpectedly negative (but insignificant) estimates for left opposition, state employees, and elections periods. Among variables for which we posed no hypotheses, the estimate for Right-led governments is close to zero and thus consistent with our discussion of government effects. The OPEC estimate is consistent with Roubini and Sachs' (1989) view that post-OPEC conditions exacerbated citizen wants. The right opposition estimate is positive.

With respect to the relative strength of effects, standardized slope estimates (betas) help clarify the relative magnitudes of effects of additive coefficients within an equation. This is especially the case in equations uncomplicated by interactions (Cohen and Cohen 1975). Thus, a ranking of the column 1 variables in terms of these betas may be instructive: aged population (.262), center government leadership (.237), state centralization and left corporatism (.238 and .221, respectively), price levels (.192), turnout (.131), state traditionalism and center party opposition (both .114), post-OPEC (.105), and left government (.104). The finding that the welfare impact of center governments exceeds that of left governments poses the one notable surprise. However, this ordering is consistent with findings and arguments in several studies (e.g., Wilensky 1981). Christian democratic and progressive liberal commitment to social amelioration (without a commensurate commitment to high-employment policies) goes far toward explaining this center government finding.

Turning to opposition–government interactions, we find five notable ones, four of them statistically significant interactions (see Table 3, col. 2). Specifically, we find negative *embourgeoisment* effects on left governments exerted by right, as well as center, oppositional strength. As center or right oppositional strength rises, left government expansion of welfare effort ebbs. In addition, we find positive "contagion from the Left" effects upon Center-, as well as Right-led governments, and marginally significant prowelfare pressures upon Right-led governments from center oppositions. With this strong evidence of the opposition–government interactions (which cause opposition estimates to denote effects of oppositions on indistinctly led, or "nonpartisan," governments) the left opposition estimate turns, somewhat anomalously, significantly *negative* while right, as well as center, opposition estimates remain significantly positive. However, the pattern is plausible: net of their impacts upon governments with distinct partisan leaderships and, in particular, governments of indeterminate leadership, left and right oppositions appear to make a "run for the middle" (Center): left parties retrench, while right ones reform (see Quinn and Shapiro 1991, on such "runs").[15]

Nonparty findings for interactive equations remain much like those for the additive equation (compare Table 3, cols. 1, 2). State employee and election period variables are insignificantly negative in both equations, calling into question the roles of reelectoral motivation and bureau voting heavily emphasized in some statist accounts of welfare state expansion. Otherwise, results are much as theory and the column 1 findings suggest. Voter turnout, electoral competition, and our dimensions of political institutions bolster welfare effort, as do affluence, price levels, unemployment, aged populations, and economic openess. Further discussion of findings, especially of the rather complex partisan ones, is in order. However, before proceeding, some diagnosis of estimate stability is wise.

First, we use the jackknife procedure to gauge stability across important subsets of cases (i.e., nations). A second procedure involves a reestimation of Table 3, column 2 after deletion of variables revealed unstable by jackknifing (Miller 1974; Tukey 1958). Consideration of estimate robustness, although tedious, will help us boil findings down to a relatively robust set.

Diagnoses

The jackknife reestimations entail (1) new estimates of slopes and t-statistics for each regressor in each of the 18 possible 17-nation subsets of the original

TABLE 3

Multiple Regression (GLS) of Welfare Effort on Hypothesized Determinants

	WELFARE EFFORT WITHOUT PARTY INTERACTIONS (1)		WEFARE EFFORT WITH PARTY INTERACTIONS		POSTJACKKNIFE WITH PARTY INTERACTIONS ("SHORT EQUATION")
VARIABLE (NAME)	EST. COEF.	STAND. COEF.^{a}	EST. COEF. (2)	JACKKNIFE (3)	(4)
Turnout	.0765* (.008)	.131	.0717* (.008)	0 6.977	.0567* (.007)
Electoral competition	1.4101* (.825)	.014	1.4590* (.833)	1 −5.841	—
State employees	−.0256 (−.018)	−.010	−.0192 (−.018)	7 −4.090	—
Election period	−.0001 (−.000)	−.001	−.0003 (−.000)	11 2.472	—
Left government	.0154* (.006)	.104	.0301* (.008)	1 4.400	.0038 (.007)
Center government	.0324* (.004)	.237	.0080 (.007)	9 .022	.0300* (.007)
Right government	.0264 (.119)	.063	−.0055 (−.008)	11 −3.919	.0135* (.006)
Left opposition	−.0016 (−.012)	−.004	−.0591* (−.018)	0 −1.585	−.0657* (−.016)
Center opposition	.0475* (.009)	.114	.0523* (.012)	0 8.723	.0706* (.012)
Right opposition	.0335* (.013)	.082	.0356* (.014)	2 3.380	−.0303* (−.012)
Left corporatism	.0132* (.003)	.221	.0138* (.003)	0 .243	.0176* (.003)
State centralization	.0142* (.002)	.238	.0133* (.002)	0 3.630	.0182* (.002)
Bureaucratic traditionalism	.0068* (.002)	.114	.0076* (.002)	0 −3.815	.0095* (.002)
Center–Left	—	—	−.0900* (−.020)	1 −5.855	−.0537* (−.012)
Right–Left	—	—	−.0585* (−.018)	3 −8.836	—
Left–Center	—	—	.0634* (.015)	1 8.044	.0294* (.009)
Left–Right	—	—	.0934* (.017)	0 79.616	.0492* (.009)
Center–Right	—	—	.0151 (.011)	4 2.191	—
lnGDP	.0118* (.003)	.062	.0130* (.003)	0 −2.401	.0103* (.003)
Price level	.0004* (.000)	.192	.0004* (.000)	0 1.001	.0005* (.000)
Unemployment rate	.1091* (.027)	.046	.1183* (.029)	0 6.234	—
Aged share of population	.6643* (.058)	.262	.6907* (.058)	0 10.611	.7159* (.061)

TABLE 3

Multiple Regression (GLS) of Welfare Effort on Hypothesized Determinants ***continued***

VARIABLE (NAME)	WELFARE EFFORT WITHOUT PARTY INTERACTIONS (1) EST. COEF.	STAND. COEF.[a]	WEFARE EFFORT WITH PARTY INTERACTIONS EST. COEF. (2)	JACKKNIFE (3)	POSTJACKKNIFE WITH PARTY INTERACTIONS ("SHORT EQUATION") (4)
Openness	.0206* (.004)	.090	.0205* (.004)	0 8.790	—
Post-OPEC	.0127* (.001)	.105	.0137* (.001)	0 6.551	.0153* (.000)
Constant	−.0690* (−.014)	.000	−.0049* (−.001)	0 −1.517	−.0232* (−.011)
Degrees of freedom	394		389		394
R^{2c}	.908		.923		.912
Adj. R^2	.904		.921		.908
DW d	1.884		1.890		1.921

[a] Standardized coefficients, which vary largely between −1.0 and +1.0.

[b] Jackknife information consists, first, of the number of times that slope estimates for a variable fail to yield absolute t-values of 1.0 or more. It secondly includes (below these counts in parentheses) *signed* t-values computed by means of jackknife procedure (Tukey 1958, 135–48). All jackknifing is done here with nations as observations.

[c] R-squareds are Buse estimates of "real" R-squared (see Kmenta 1986, 616–85); Adjusted R-squareds are computed from these; and Durbin–Watson d is computed on corrected residuals and adjusted for "gaps" (or international variation) in stacked time series.

*$p \leq .05$, one-tailed test.

sample and (2) unique new jackknifed estimates of slopes and t-statistics for each variable for the complete population of cases (computed from the new estimates described in point 1).[16] We display information of type 1 in terms of counts of the number of times that t-statistics for given variables fall below 1.0 in absolute value. We display information of type 2 in terms of signed t-statistics for regressors, for these illuminate both the direction and statistical significance (and robustness) of coefficients. (See Table 3, col. 3 for jackknife results.)

The jackknife procedure suggests that several variables may have cross-nationally unstable estimates. First, electoral competition has an anomalously negative t-value of −5.841, while state employee and election period variables have numerous t-values less than 1.0 in absolute value. In addition, bureaucratic traditionalism and economic product both have anomalously negative jackknifed t-statistics (despite 18 correctly signed ones in each of the 18 subpopulations).[17] Left and center opposition terms and the Left–Right product terms show slight signs of instability (one or two "small" t-values each). The center and right government, right opposition and the Right–Left and Center–Right interactions show more troublesome signs of instability; that is, they display several "small" t-values and/or anomalously signed jackknifed statistics.[18]

Table 3, column 2 probably represents the most informative of our equations.[19] Nonetheless, examination of an equation from which cross-nationally unstable regressors have been removed has the possible advantage of deleting arguably extraneous variables and the certain advantage of highlighting ones on which evidence is robust. To identify such an equation, jackknife computations (some reported in col. 3) were used to specify the cross-nationally robust equation of column 4.[20]

This equation retains four of the previously six "environmental" variables: affluence, prices, aged population, and post-OPEC (but not unemployment or openness). It bears 13 of our 18 manifestly political variables: voter turnout, the three political-institutional dimensions, all six governmental and oppositional measures of party strength and three opposition–government interactions [Center–Left, Left–Center and Left–Right]). By omitting variables that are conventionally "relevant" to the prediction of welfare effort (despite their reliance on particular cases for relevance), the "short" equation does court specification bias. Yet, the short equation has heuristic value for highlighting the variables that withstood jackknife diagnoses even though it does not supplant the column 2 equation as our best-specified, least-biased, best-fitting, and, indeed, most informative equation. Although we must discount the estimates that do not make it past the jackknifing procedure into the column 4 equation, the column 2 equation has the best estimates.

Final patterns

Two forms of "contagion from the Left" are evident across all equations. In one, the strength of left parties excluded from governmental leadership pressures Right-led governments to augment welfare effort (see Figure 1, upper left-hand quadrant). Right government effects on welfare effort are negative (−.0055) in the absence of all left opposition (e.g., in the United States) in the preferred equation of column 2. However, they become positive with increases in left opposition (see Figure 1, upper left). Principal examples of strong prowelfare effects of right governments facing strong left oppositions occur for Australia and New Zealand in the 1960s (when left oppositional strength figures at times approached 49%) and Japan during late 1960s and 1970s when left oppositional strength peaked. In the second form of "contagion from the Left," left opposition effects on center governments' welfare effort roughly parallel those on right government welfare effort (see Figure 1, upper right). Principal examples of upward boosts in center welfare effort under the influence of strong left oppositions are Finland in the 1960s and, more fleetingly, Norway in 1970–72 and Sweden in 1981–82. (The most salient *lapses* of center party welfare effort where left oppositions are absent occur in the United States during the Carter and (late) Johnson administrations.)

Two forms of left *embourgeoisment* effects are present. Moderations of the prowelfare effects of Left-led governments by centrist oppositions and junior partners are present in all equations. In the equation of Table 3, column 2, left government effects, strongly positive (.0301) when center oppositions are null, decline quickly with increases in the strength of center oppositions, approaching zero as center "oppositions" reach high shares of seats and votes (i.e., 45%; see Figure 1, lower left-hand quadrant). The greatest moderations (and possible reversals) of left welfare effort under centrist pressures occur in Austria and Sweden. The second form of left *embourgeoisment* effects reinforces this picture of substantial, but readily compromised, left government support for welfare effort. As the lower right quadrant of Figure 1 indicates, our findings include a Right–Left interaction indicating that prowelfare effects of left governments are strong at the outset but decline toward zero (and beyond) as right "opposition" shares of votes and seats approach 45%. The greatest such attenuations of left welfare effort arise for the German Social Democratic party governments of the 1970s and Danish left governments of the early 1960s.

Overall, strong evidence emerges for strategic interactions among parties. In these interactions, dominant governing parties accommodate their policymaking orientations to those of parties in oppositional—and subordinate governmental—roles. Some interesting surprises emerge from these interactions. Left parties appear to submit to strong antiwelfare pressures from *all* oppositions. Despite typically clear left rule effects on welfare effort and favorable centrist orientations toward welfare effort when in office, center oppositions appear to dampen the welfare effort of left parties. Perhaps this occurs because the strong, longer-run welfare-spending legacies captured by our left corporatism variables can be effectively evoked by the center to claim middle-of-the-road swing voters. On the other hand, centrist welfare orientations are reinforced, rather than undercut, by left oppositional pressures.

Returning to nonpartisan variables, evidence for positive voter turnout effects is pervasive and robust. Evidence for prowelfare effects of political institutions is also strong. Significant positive estimates for social democratic corporatism, state administrative/political capabilities, and traditional policy legacies is everywhere strong and robust. Finally, evidence for the roles of socioeconomic forces is strong: affluence, the sizes of aged and unemployed populations, inflation, openness, and the increased material needs of the first post-OPEC decade all exert upward pressure on welfare effort.

CONCLUSION

Both electoral turnout and differences in the strengths and ideologies of parties matter for the share of national incomes spent by democracies on social welfare programs. Moreover, not only does the strength of governing parties matter, but the strength of oppositional parties and parties at the margins of ruling coalitions plays an important role in determining welfare effort. Dimensions of political institutions that include traditions of left party electoral success and working-class mobilization and interest representation, state administrative and political capacities for cohesive policymaking, and heavily bureaucratic and traditionally hierarchical state institutions also matter, along with a host of socioeconomic environmental conditions. However, neither the preelectoral budgetary machinations of incumbent politicians nor the bureau voting of public sector interest groups (each a focal activity in public choice theory) seems to matter for welfare effort. Similarly, party electoral competition, a centerpiece of much "democratic pluralist" theorizing, does not seem to matter for welfare effort.[21] These conclusions appear to hold, at least for the 18 largest affluent democracies between 1960 and 1982.

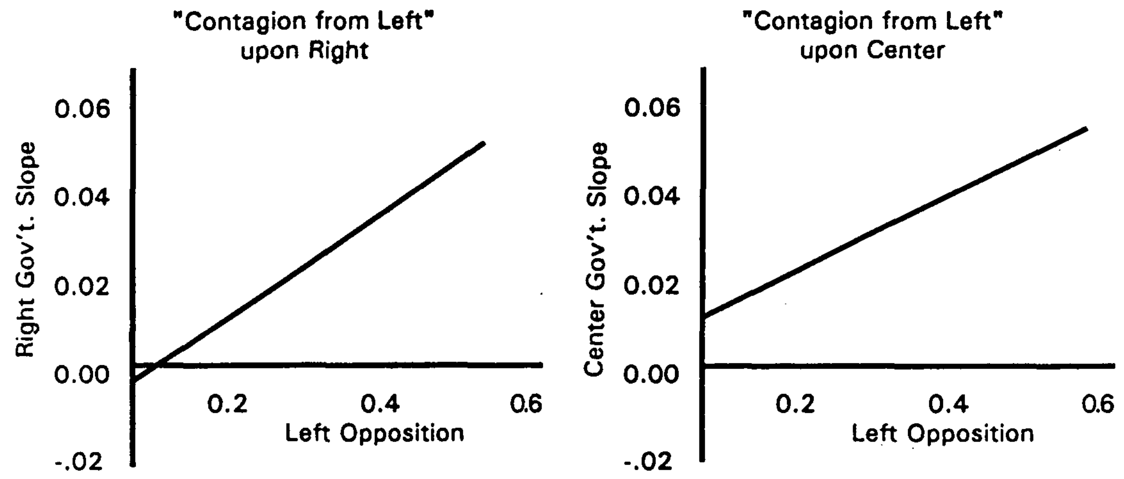
Consistent with recent claims by Pampel and Williamson (1989), voter turnout appears to have exerted an upward pressure on welfare effort. Even net of ample controls for political and environmental factors, the political democratic stress on turnout as a source of demands for social ammelioration is vindicated.

Consistent with partisan theory stressing center party delivery of income security outlays, our simple additive models indicate that Center-led governments on average generate higher levels of welfare effort than do other governments, left ones included (Wilensky 1981). However, consistent with the social democratic authors who acknowledge centrist as well as leftist advocacy of income security spending, our additive findings encompass clear left party effects on welfare effort (see Castles 1982; Stephens 1979; see also our earlier work, Hicks and Swank

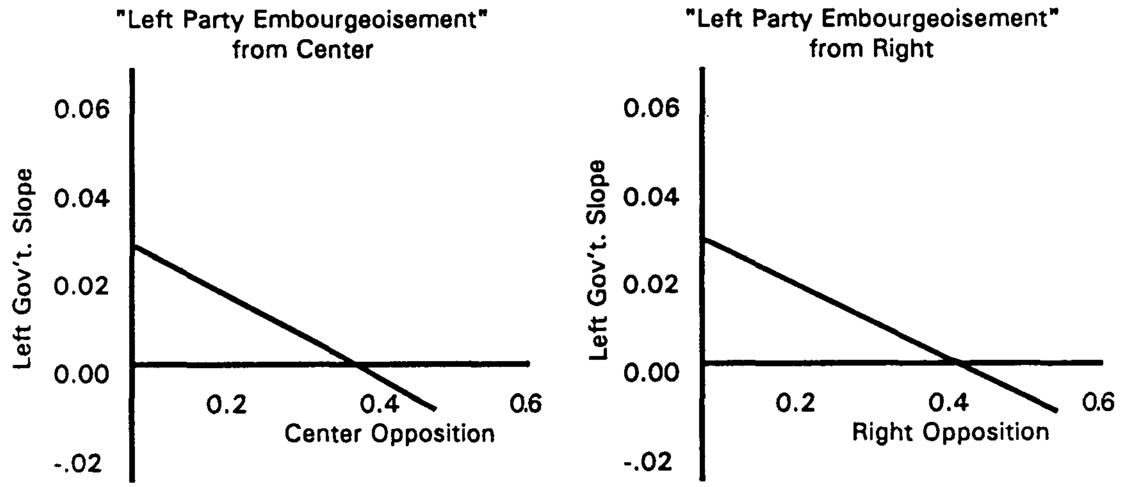
FIGURE 1

Selected Conditional Slope Estimates for Types of Governments Plotted Against Contingencies

"Contagion from the Left" Plot[a]



Left Party Embourgeoisement Plot[b]



Note: [a]Estimates of opposition-contingent effects of partisan governments when oppositional conditions for them equal .0 (e.g., of right government effects when left opposition = 0) are the slope estimates attached to the relevant partisan government term (e.g., right government) in the estimation equation. Estimates for slopes of lines in our plots (e.g., for effects of right government *by* left opposition) are the slope estimates for the product terms (like Left–Right) for the pair. Further elaboration of interactions are available on request; see also Friedrich 1982; Lange and Garrett 1985.
[b]Partial plots of left governments' effects conditional on center opposition controlling for right opposition (on left of panel) and of left effects conditional on right opposition controlling for center opposition (on right).

1984). The finding that center effects not only complement left ones but actually exceed them should be no great surprise. Lack of consistent center (as opposed to left) full-employment policies has made centrist governments dependent upon income transfer programs for their pursuit of income security (Alvarez, Garrett, and Lange 1991; Janoski 1990). Furthermore, centrist welfare effort appears to be intensified by left oppositions and impervious to right ones, while left welfare effort appears to be dampened by center, as well as right, oppositions. In short, oppositional dynamics appear to buoy center party welfare effort but to depress left party welfare effort. Moreover, inclusion of left corporatism, a broad-ranging measure of left political strength, into our models slightly moderates estimates for short-run effects of left governments. Whether this moderation should be regarded as suppressing or sharpening the short-term effect of left government is unclear. What is clear is that our present introduction of left corporatism extends—and thus augments, rather than limits—overall evidence for the policy relevance of social democracy.[22]

In interactive models, Left-led governments appear to affect welfare effort in inverse proportion to the strength of center and, perhaps, right oppositions, augmenting it when and where these oppositions are relatively weak and reducing it insofar as they are strong. Patterns of left government accommodation to oppositional strength pervade these analyses. (Whether left governments compensate victims of welfare retrenchment with alternative benefits we cannot say.) Complementing the patterns of left accommodation to center and right oppositions are patterns of "contagion from the Left." To the extent that left oppositions are electorally and legislatively strong, they pressure parties to their *right* to enhance welfare effort. As left opposition strengthens, otherwise weak or negative social spending impacts of right governments are lifted to substantially positive levels. Notable center party contributions to welfare effort are substantially multiplied when the Left is strong. Interestingly, governments without clear left, conservative, or centrist leadership appear to be as supportive of welfare effort as conservative governments. In addition, left and right oppositions appear to affect welfare policy in additional ways. Net of our evidence for oppositional pressures, we find indications that left and right parties also may sometimes woo voters and, when governments lack distinct partisan leadership, court inclusion in ruling coalitions by reversing characteristic welfare stands. Consistent with Quinn and Shapiro's (1991) "run for the middle," left oppositions sometimes facilitate welfare retrenchments, while right parties sometimes foster welfare expansion.

In short, the partisan character of governments, oppositions, and interactions between the two matter for welfare effort. Overall, the importance of left and center governments for the extent of welfare effort revealed by our simpler, additive findings reaffirms earlier findings (Castles 1982; Hicks and Swank 1984; Stephens 1979).[23] With regard to the interaction of governments and oppositions, our findings suggest a mix of party distinctiveness and interparty convergence in the policy enactments of party governments that resembles claims by recent Downsian theorists (e.g., Austen-Smith and Banks 1990). However, interparty convergences in our models of welfare effort are functions of specific differences of ideology and strength between governments and oppositions, such as are not highlighted in Downsian theory. True, our discussion of convergence is formulated in terms of a set of loosely coupled propositions (e.g., "contagion from the Left") that might benefit from, as well as inform, formal theorizing within the Downsian tradition. However, our treatment of these questions implies that extension of the Downsian tradition to the comparative study of policy determination may require some reformulation of conventional Downsian models.

Finally, our analyses suggest that political institutions matter a great deal for welfare effort. Cross-national differences in the three dimensions of political institutions identified here substantially differentiate states' social welfare effort provision. Social democratic corporatism, conceived in terms of the degree to which left parties and unions mobilize and incorporate industrial workers into national policy-making networks, appears, net of short-run partisan politics, to be a strong source of national differences in welfare effort. In addition, structurally centralized states with histories of early consolidation of major social security programs do, indeed, appear to capacitate national welfare effort, as many have suggested (Orloff and Skocpol 1984; Wilensky 1975). Lastly, bureaucratic traditionalism—a combination of a highly bureaucratized welfare administrations plus hierarchical, antidemocratic legacies, that apparently undergirds welfare state clientelism (Esping-Andersen 1990; Rokkan 1970)—enriches our conception of political institutions and further differentiates the welfare efforts of contemporary industrialized democracies.

Appendix:

Y *Welfare Effort.* Total social welfare expenditures as proportions of gross domestic product, from the ILO's *Cost of Social Security.*

X_1 *Turnout.* Valid voters as proportions of persons eligible for voting, from Mackie and Rose (1974); *European Journal of Political Research,* (selected years). Measurement at $t - 1$ in analyses.

X_2 *Party Competition.* Measures of competition among parties computed from data on mass voting for parties, as in Pampel and Williamson 1989, chap. 3 and Swank 1983. Sources are the same as those for X_1 and X_5 through X_{10}. Measured at $t - 1$ in analyses with the formula

$$1 - \sum_{i=1}^{N} (P_i - 1/N),^2$$

where N is number of parties and P_i is proportional vote for party i.

X_3 *State Employees.* All government civilian employees as proportions of population with data from OECD 1982, OECD's *Labor Force Statistics,* and various national statistical yearbooks. Measured at $t - 1$ in analyses.

X_4 *Election Period.* Equal to 1 if a year is an election year (in which presidential or general parliamentary elections occur) *or* a preelection year; otherwise 0. Election times from *Keesings Contemporary Archives* (selected years), and Mackie and Rose 1974; *European Journal of Political Research* (selected years).

X_5 *Left Party Government Leadership.* Four-year average ($t - 4$ through $t - 1$) of left party leadership of government, coded 1 for each year in which left party cabinet participation (portfolios as percentage of total cabinet portfolios) is more than 120% of center party participation and more than 120% of right party participation, otherwise 0.[24] Party classifications generally correspond to Castles and Mair 1984, although some minor divergences exist. On these differences see Swank 1988, 1992. Cabinet participation data come from Browne and Dreijmanis 1982 and *Keesings Contemporary Archives.*

X_6 *Center Party Government Leadership.* Strictly analogous to left party government leadership.

X_7 *Right Party Government Leadership.* Strictly analogous to left party government leadership.

X_8 *Left Opposition.* Electoral and legislative strength of left parties (as categorized by Castles and Mair 1984) when not leading government and measured as an average of left party proportion of vote in last election and proportion of seats in lower house of legislature as ascertained from Mackie and Rose (1974), *European Journal of Political Research* (selected years). Averaged $t - 4$ through $t - 1$. Equal to 0 when left government leadership equal to 1.

X_9 *Center Opposition.* Strictly analogous to left opposition.

X_{10} *Right Opposition.* Strictly analogous to left opposition.[25]

X_{11} *Left Corporatism (Factor 1).* Factor-score scale measuring strength of long-term working-class mobilization and political economic integration (see Table 2). Union strength is measured using a modification of Cameron's index of union strength (1984, 165). This consists of the product of union density (in 1960) times the average of *three* scales of union centralization: organizational unity of labor, confederation power in central bargaining, and scope of collective bargaining. In order to stress the importance of union centralization and compensate for possible exaggeration of union centralization by Cameron in Denmark and the low countries, union centralization is measured a second time by means of a three-point (one-, two-, or three-level) scale devised by Wallerstein (1989). Left party strength is measured following Stephens's (1979) index of cumulative rule, as the number of years, 1946–60, marked by left party–led government. Left voter mobilization is measured as an index of left voting weighted by voter turnout, specifically as $[(LPV + LCV)/2] \times TO$, where LPV is *left party votes* as a proportion of all votes (1957–60), LCV is Korpi's (1983) index of *class voting* around 1970 (assumed adequately to approximate cross-national variations in 1960 class voting), and TO is the average, proportional voter *turnout* for 1957–60.

X_{12} *State Centralization (Factor 2).* Factor-score scale measuring state centralization, central state prominence, and legacies of welfare program consolidation (see Table 2). In brief, state centralization is measured in three ways: (1) as a unitary/federal (UF) dichotomy (unitary = 1.00, otherwise .00) (see Lijphart 1984, Table 10.2 for categorization); (2) in terms of revenue centralization (RC) as the ratio of central governmental to all governmental revenues extracted in 1960 (see Lijphart 1984 and Pampel and Stryker 1988 for precedents and OECD's *National Accounts of OECD Nations* for data); (3) as a composite index of state centralization weighted by the size of the state centralized. Specifically, it is measured as $[(UF + RC)/2] \times GE$, where GE is the *government employee* share in total employment, UF and RC are as defined, and all three elements of the index vary between .0 and 1.0 (see Table 2 on state centralization). Following Pampel and Stryker 1988, state bureaucratization is measured by stressing administrative costs of welfare spending as proportions of all welfare spending. Welfare program consolidation measures the legacy of a diversified welfare state by the number of years up to 1960 that a nation has had *all* of the following four major welfare programs: (1) social security retirement pensions; (2) unemployment compensation, (3) workman's compensation and (4) sickness and maternity care. Family allowances were dropped because they had still not been adopted by all affluent democracies by 1960. Of course, health programs were not universally adopted at the minimal level recorded by Social Security Administration (1981) data sources until the mid 1960s; but the number of post-1960 "years-to-adoption" is small and makes for minor measurement error. The pluralist concern with formal political institutions and their majoritarian/consensus interpretation is operationalized using the factor scores for Lijphart's majoritarian minimal winning coalition and majoritarian unitary factor-analytical dimensions (1984, chap. 13).

X_{13} *Bureaucratic Traditionalism (Factor 3).* Factor-score scale measuring extent of state traditionalism, bureaucratization, and majoritarian unitary government. State traditionalist legacy is measured as an additive index of three (standardized) items: (1) a five-point scale of the *extent of state absolutism* in the eighteenth and nineteenth centuries (Rokkan 1970, chap. 3, Table 1); (2) a five-point scale of *resistance to mass enfranchisement* (ibid., Table 2); and (3) a three-point scale of the extend of class rigidity (class systems as systems of status division; Lipset 1983). Although a straightforward, unweighted additive index was used, a principal components analysis indicated unidimensionality by yielding the following pattern of loadings: absolutism (.81); obstructed enfranchisement (.84), class rigidity (.80). Bureaucratization is measured as for X_{12}, and majoritarian unitary government as in Table 2 and Lijphart 1984.

X_{14} *Natural Logarithm of Real Gross Domestic Product/Population (lnGDP).* Through 1980, from Summers and Heston 1982. Thereafter computed as extrapolation of 1980 figures from growth rates from OECD 1986. Measured at $t - 1$.

X_{15} *Price Level.* Consumer price index from International Monetary Fund. Measured at $t - 1$ in analyses.

X_{16} *Unemployment Rate.* Unemployed as proportion of economically active population at t and $t - 1$, from OECD's *Main Economic Indicators*.

X_{17} *Aged Share of Population.* Proportion of population over 65 years of age, from United Nations' *Demographic Yearbook*. Measured at year ($t - 5$ through $t - 1$) with highest correlation with welfare effort in "aged" regression model.[26]

X_{18} *Openness.* Imports plus exports as proportions of gross domestic product, from OECD's *Main Economic Indicators*. Measured at $t - 1$ in the analyses.

X_{19} *Post-OPEC.* Equal to 1 for 1975 and later, otherwise 0.

Notes

We wish to acknowledge the helpful comments of John Freeman, Tom Lancaster and Arend Lijphart on drafts of this paper, as well as participants in the Conference on the Welfare State in Transition, Bergen, Norway (August 1989); a Welfare State Study Group seminar, Center for Policy Research and Urban Affairs, Northwestern University, Evanston, Illinois (January 1990); and the Department of Sociology seminar Emory University (Fall 1990). We also wish to acknowledge the industry of half a decade of research assistance, especially from Clark Denny, Richard Lee, Joya Misra, John Pitt, and Wesley Chapin and the generous typing of Cathy David and Margaret Stephens.

1. See Hicks, Swank, and Ambuhl 1989 and Pampel and Williamson 1989 for conflicting views on partisanship; see Garand 1988 and Korpi 1989 for conflicting views on state administrators; see Dye 1979 and Hicks and Swank 1984 for conflicting views on electoral factors.

2. As for Bismarckian "federalism," Taylor debunks it best: "Bismarck's constitution was supposed to be federal, but its federalism was fraudulent, window dressing to make the dictatorship of Prussia more respectable" (1946, 117).

3. Each of these perspectives acknowledges the welfare relevance of social welfare needs, economic capacities for welfare provision, and economic structure. Note that we attempt in our models to specify only *direct* (or "structural") influences upon policy. For example, party-channeled effects of openness suggested by Cameron (1978) are ignored.

4. There is a strong theoretical and empirical basis for ascribing center parties prowelfare policy goals. Baldwin's (1990) study of Scandinavian and British "welfare" politics highlights the middle-class appeal and secular center advocacy of social insurance policies as anodynes for middle-class risk while Stephens (1979) and Wilensky (1981) analyze and document similar consequences of the Christian (democratic) center.

5. For a detailed description of the items used to measure left corporatism, state capabilities, and traditionalist policy legacies, see X_{11}–X_{13} in the Appendix. Although these institutional measures are temporally invariant, they are not exempt from causally legible associations with welfare effort. We confine measurement to circa 1960, where strong temporal invariance can be assumed; and we extend it back earlier to wherever temporal variability or historical depth mandates

(e.g., to 1946 for left rule; to the eighteenth century for absolutism) and wherever data permit.

6. Age was measured for each nation at the year $t - 5$ through $t - 1$ for which the population share over 65 years of age most strongly affected welfare effort. Almon distributed-lag regression of effort upon population share at $t - 5$ through $t - 1$ was used to identify key years (Johnston 1984). Census use of interpolated values for annual population may, alas, offset our effort at precision. Also, economic product was measured as the natural logarithm of per capita gross domestic product because this defines slopes per unit of percentage change in "product," rather than per raw dollar changes in it and such changes are more likely to be linearly related to welfare effort (a proportion) than are changes in raw dollars: a change from $4,000 to $5,000 should matter more than one from $14,000 to $15,000.

7. For OPEC, an arguable fourth exception, years were coded "post-OPEC" starting in 1976 because simultaneous deterioration of income growth, inflation, and unemployment did not occur until 1975, and one-year response lags seemed apt.

8. Our measurement of party strength diverges from common single-year measures (e.g., Swank 1992). It particularly diverges from the "1946-to-present" cumulative measures in Pampel and Williamson (1989), which overreact to the limitations of "immediate" party effects with 35-plus year measures and *equate* the policy influences of recent, with those of decade-old, governments.

9. A regression was run with the regressors of the "basic" model and the Almon distributed lags for $t - 6$ through t for government and opposition items.

10. This seemingly permissive criterion was adopted after a more stringent one (e.g., greater than 50% of all cabinet portfolios) yielded more periods of indeterminate governmental leadership than was plausible. See also n. 24.

11. This measure combines strictly oppositional parties entirely excluded from governmental participation with junior partners in coalition governments, distinguishing for the first time the strengths of clearly governing parties and other parties. Note that for the typical two-house legislature, the lower house was used to focus measures for both votes and seats.

12. We have a further reason for excluding indeterminate governments: it allows for simultaneous tests of interactions for all three major types of parties. Also, interpretive focus, statistical tractability and numerical relevance ground our grouping of oppositional and junior partner parties in the measure of oppositional strength. Separate specifications of oppositional, junior partner, and leadership variables across left, center, and right parties (for nine terms) would exacerbate multicollinearity and complicate interpretation very seriously—prohibitively once interactions (as many as 12) were introduced. Grouping of governmental junior partners with governmental leaders would blur our focus on dominant partisan directions of governments. Finally, the legislative and electoral strength of junior partners relative to strictly oppositional ones is small (on average .02 as opposed to .17 per party type); and the variance explained by partner strength as opposed to strictly oppositional strength in the current oppositional measure is small as well (on average, 10%, as opposed to 90%). Note that ommission of (any) one of the three ostensibly redundant oppositional variables does not notably alter findings.

13. Detailed elaboration of the interaction model is available on request. See also Friedrich 1982.

14. We do so by excluding the measure for the benchmark category from the equations. We enter all six of the possible opposition–government interactions, then drop the one (with the *lowest* t-statistic) that does not have an (absolute) t-value greater than 1.0 (see Johnston 1984, 504–6). Then we repeat this procedure until only interaction terms with (absolute) t-values greater than 1.0 are retained. As a check on estimate stability, we also proceed with a forward, stepwise treatment on the interactions for both equations. This yields identical results. Yet we must caution that multicollinearity levels for these variables measured in terms of R-squared deletes (or "1-tolerance") are never less than .90. Hence, we cannot confidently assert that t-tests have the power for exclusions of opposition–government interactions to be compelling, only that retentions override serious loss of power due to multicollinearity and evince indications that multicollinearity has really taken a bite. As for eventually excluded variables, we cannot be certain that they do not result from multicollinearity.

15. Opposition findings are unaltered by deletion of any single opposition term, despite resulting reductions in multicollinearity (see n. 12, 14).

16. This procedure is executed as follows for the equation of Table 3, column 2. The equation is reestimated with each of its 18 nations removed in one of the 18 reestimations. Reestimations are examined qualitatively for departures from the estimates obtained in the original estimations with all 18 cases. Reestimations are also used to compute new jackknifed values of coefficient estimates (BJ) plus new test statistics for each new jackknifed coefficient. A given jackknifed coefficient estimate BJ equals the mean of the "pseudovalues" B_i*. $B_i* = 18(B) - 17(B_i')$, where B is the 18-case estimate of the coefficient in question and the B_i' are the 18 separate 17-case estimates (and $i = 1, 2, \ldots, 18$). The standard error for each of the BJ estimates is just the standard error of the B_i* that are averaged to define BJ. From the values of the BJ and their standard errors, t-statistics are computed.

17. As pseudovalues equal $B_i* = 18(B) - 17\ (B_i')$, where B is the 18-case estimate of the coefficient in question and the B_i, are the 18 separate 17-case estimates (and $i = 1, 2, \ldots, 18$), they may be distinctly signed from both the initial sample-wide and subsample estimates where the absolute values of $17(B_i')$ exceed those of $18(B)$. Since the jackknifed slopes (standard errors and t-values) are computed from the pseudovalues, jackknifed estimates may be marked by reversals of signs from full-sample and subsample estimates.

18. However, it merits noting that some of these variables were already insignificant in Table 3, column 2, and that no new instability is revealed for these. Moreover, although statistically insignificant estimates may be viewed skeptically, *significant* interactions are dubiously interpreted in the absence of additive estimates for their component parts (Friedrich 1982). These estimates anchor conditional effects of partisan governments to intercepts, as well as to levels of oppositions. For example, where left government and right oppositions interact, slope estimates for the left government term anchor the variation of left government effects (across levels of right opposition) by specifying these effects when right opposition is .0.

19. A variant of the column 2 equation that substituted the OECD measure of social welfare transfer expenditures for the ILO measure of social security spending (including in-kind benefits and services) yielded few new indications of instability. A dummy variable–generalized least squares variant was also run. This controls for 0–1 dummy variable regressors for 17 of our 18 nations, leaving strictly longitudinal, within-nation variance for other regressors to explain. It provides some immunization against any exclusions of causally relevant, cross-national differences from the matrix of regressors; but it also downwardly biases (in absolute value) all regressors with cross-national components to their variation, because it monopolizes all cross-national variation (Judge et al. 1985). This variant reinforces evidence of estimator stability.

20. To jackknife, all nonparty variables with t-statistics below 1.0 in absolute value in the jackknifed result of Table 3, column 3, were deleted; and jackknife computations were performed on the resulting specification. Then, using results of these jackknives, nonparty and party product (or interaction) term variables with absolute t-values below 1.0 were deleted, and jackknife computations performed again. As above, we refrained from removing additive "party" specifications with t-values below the 1.0 criterion level because of their integral part in the interpretation of interactions (see nn. 13, 18). Pertinent left government, right government, and

right opposition variables have significant jackknifed slopes and may well be "stable."

21. An anonymous reviewer suggested that the median voters for whom parties compete sometimes favor welfare retrenchment *and* that center parties represent broad middle-class constituencies (cf. "risk-class?". Baldwin 1990). This pair of insights offers a good puzzle for future study.

22. For a few years, findings from Pampel and Williamson (1989) have clouded "partisan–welfare" relations. They coded their party variables so that first center, and later nonright, governments serve as benchmarks. Their analyses reveal that left welfare effort is less than center effort (chap. 3, Table 3) and center less than nonright (chap. 3, Table 6); but their analyses do not reveal whether or not left welfare effort exceeds right effort (as in, e.g., Hicks and Swank 1984; Stephens 1979). Moreover, that study suffers from an unnecessarily small and arbitrary sample of 126 observation (18 every five years, 1950–80) and a rather thin specification of equations. Certainly, ours are the first *clear* findings of distinct yet "less-than-center" effects. As for left corporatism's impact on year-to-year left rule effects, optimal precision may call for a less governmentally centered specification of left corporatism. On the other hand, precision might warrant a yet fuller specification of nonleft traditions of political economic strength. Present specifications of policy-relevant institutions should not be regarded as definitive.

23. See note 21.

24. *Party Leadership. Australia* conservative 1960–72 and 1976–82, left 1973–75; *Austria* center 1966–69, left 1970–82, none 1960–65; *Belgium* center 1960–71, 1974–80, none 1972 and 1981–82, left 1973; *Canada* right 1960–63 and 1979–82, center 1963–78; *Denmark* left 1960–67 and 1972–82, right 1968–71; *Finland* left 1967–72, 1973–75, none 1964 and 1971, center 1960–63, 1965–66, and 1976–77; *France* right 1960–80, left 1981–82; *West Germany* right 1960–66 and 1982, none 1967–69, left 1970–81; *Ireland* right 1960–82; *Italy*: center 1960–82; *Japan*: right 1960–82; *Netherlands* center 1965–66 and 1978–82, left 1974–77, none 1961–64, 1967–72, and 1973; *New Zealand* right 1960–72, 1976–82, left 1973–75; *Norway* center 1966–82, right 1982, left 1960–65, 1971–72, and 1974–81; *Sweden* left 1960–75, center 1977–82; *Switzerland* center 1960–82; *United Kingdom* left 1965–69 and 1974–78, none 1970, right 1960–64, 1971–73, and 1979–82; *United States* center 1960–68 and 1977–80, right 1969–76 and 1981–82. For rationales and summary of leadership periods, see n. 11.

25. The product terms for opposition–government interactions are center opposition × left party government leadership (Center–Left), right opposition × left party government leadership (Right–Left), right opposition × center party government leadership (Right–Center), left opposition × center party government leadership (Left–Center), left opposition × right party government leadership (Left–Right), and center opposition × right party government leadership (Center–Right).

26. See n. 6 for operational details.

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RESTRICTIVE LEGISLATIVE PROCEDURES IN FRANCE AND THE UNITED STATES

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I develop a framework for applying existing formal models of restrictive amendment procedures in Congress to the study of the French government's use of two restrictive legislative procedures, the package vote and the "guillotine". I test six hypotheses derived from existing formal models and from existing research on the French National Assembly. The analysis shows that the French government invokes the two procedures on the same types of distributive and jurisdictionally complex bills that frequently receive closed rules in Congress. The analysis also shows that the decision to use the restrictive procedures is strongly linked to the majority status of the government, suggesting they are used to preserve agreements between parties in the same way that restrictive amendment procedures are used to preserve agreements between individual members of Congress. Thus, existing formal models of legislative institutions can help us study how procedural structures shape strategic bargaining between political parties in parliamentary systems, especially during coalition and minority government.

Scholars widely recognize the fundamental role of formal institutional arrangements in democratic processes (Dahl 1989, pt. 4; Mayhew 1974; Riker 1980, 1986); and formal models of legislative choice have carefully explored how specific institutional structures shape legislative strategies and policy outcomes in the U.S. Congress. These models provide important insights into a wide variety of topics, including how institutional arrangements affect the distribution of benefits to congressional districts (Baron and Ferejohn 1989a, 1989b; Ferejohn, Fiorina, and McKelvey 1987; Shepsle and Weingast 1981; Weingast 1979); how institutional arrangements affect opportunities for congressional committees to exercise power against Congress (Denzau and Mackay 1983; Shepsle and Weingast 1987; Weingast and Marshall 1989); how institutional arrangements affect the efficient allocation of public resources (Baron 1990; Shepsle and Weingast 1984a; Weingast, Shepsle, and Johnson 1981); and how institutional arrangements encourage members of Congress to become policy experts (Gilligan and Krehbiel 1987, 1989). But although these formal studies make key contributions to our understanding of congressional politics, they have not been used to study how institutional arrangements shape legislative processes in parliamentary systems.[1]

The parochial application of existing formal models to Congress is unfortunate. On the one hand, comparative studies of parliamentary democracy suffer because they neglect the insights that emerge from the formal literature. On the other hand, formal studies of legislative institutions suffer because the failure to test existing models outside of Congress makes it difficult to determine if existing formal models in fact produce *general propositions* about institutional arrangements and choice processes. Consequently, claims about the comparative properties of institutional structures are difficult to defend; and theories about the choice between competing institutional arrangements are difficult to develop.

This research extends the application of formal models of legislative choice to the study of parliamentary politics. It focuses specifically on two procedures—the package vote and the "guillotine"—that sharply restrict the legislative role of the French National Assembly. I argue that existing models of congressional committees and closed amendment rules can be used to make predictions about the conditions that lead the French government (i.e., the prime minister and his cabinet) to invoke these two restrictive procedures. I then demonstrate that the French government's use of the package vote and the guillotine to resolve "distributive" problems is similar to the Congress members' use of closed rules to preserve distributive agreements. Since the logic underlying existing formal models of Congress can be used to explain the French government's use of restrictive procedures, the analysis suggests that future studies of parliamentary politics should look beyond the traditional focus on how institutional arrangements structure the hierarchical relationship between the executive and the parliament to a more general focus on how institutional arrangements constrain strategic interactions *between* parties.

THE PACKAGE VOTE AND THE GUILLOTINE IN FRENCH POLITICS

As part of an effort to redress the problems of cabinet instability and legislative paralysis that were endemic to the French Fourth Republic, the authors of the Fifth Republic Constitution created a variety of institutions intended to enable the government to restrict the legislative role played by the National Assembly. The constitutional assault on the National Assembly focused to a large extent on the right of deputies to amend legislation. Under the rules of the Fifth Republic, deputy amendments cannot have the net effect of increasing spending or decreasing revenue (Article 40); deputy amendments can be declared inadmissible if they have not been submitted to the relevant standing committee for consideration (Article 44); and deputy amendments must be in the "domain of law" rather than the "domain of regulation" (Articles 34 and 41). In addition, the Fifth Republic Constitution established two constitutional procedures, the package vote and the "guillotine," that permit the government to exclude otherwise admissible amendments. The package vote, found in Article 44.3 of the Constitution, permits the government to group articles and amendments selectively, excluding the amendments to which it is opposed. Under this procedure, the parliament must vote either to accept or to reject the government's policy. The *guillotine*, found in Article 49.3 of the Constitution, permits the government to "engage its responsibility,"[2] that is, to attach the fate of a bill to a censure vote in the National Assembly. Under this procedure, there is no vote on the bill itself. Instead, all debate ceases immediately; and if a motion of censure is not introduced and voted within a specified time limit, the bill is considered as adopted in the form designated by the government.[3] The package vote and guillotine, then, are both institutions that force the National Assembly to make an "up or down" choice on a policy package determined by the government.

Existing general studies of French politics treat both the package vote and the guillotine as part of the government's large procedural arsenal that renders the French parliament impotent (Andrews 1982; Ehrmann 1983, chap. 10; Williams 1968; Wright 1989, chap. 7). The only studies that focus *specifically* on the package vote and the guillotine are in French, and the most common theme in these studies relates to the "antidemocratic" effects of both institutions. The French studies criticize the institutions because they permit the government to protect its bills from changes by deputies in the National Assembly. Hence, the procedures move final policy outcomes away from the preferences of the directly elected deputies toward the preferences of the government. This usage incites complaints typified by that of Senator Guille, protesting in Senate debate the use of the package vote in 1962: "[The government] leaves us with a frustrating obligation, either to consent to what we judge bad and dangerous for France, or to reject what we estimate necessary and urgent for the French" (quoted in Avril 1965, 441).

The guillotine is generally perceived to be more antidemocratic than the package vote because, in addition to limiting parliamentary amendment activity, the guillotine permits laws to be adopted without the parliament's voting on them. Deputy Paul Reynaud's 11 January 1961 letter to Prime Minister Michel Debré sums up this criticism: "The promulgation of [Article 49.3] violates two principles which are at the core of all democratic regimes. . . . The first is formulated . . . by Article 3 of the Constitution: 'The national sovereignty is held by the people, who exercise it by their representatives or by way of referendum'. The second . . . is found in Article 34: 'The law is voted by Parliament'. These two principles were violated thanks to Article 49 of the Constitution."[4]

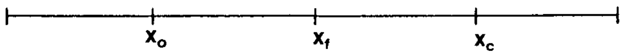
Reynaud was protesting the government's use of Article 49.3 to pass a bill relating to the *force de frappe*, which established France's nuclear weapons capability. Many deputies were hostile to the bill, and the government invoked the guillotine during the debate on a motion to send the bill back to committee before the bill was publicly debated in first reading. The government later invoked Article 49.3 during the second and third readings, so the *force de frappe* became the first bill ever to be adopted without a vote in the National Assembly (Lascombe 1981). In general, the procedure has been used on a wide variety of important bills, including bills related to the budget, social security, changes in French electoral law, the nationalization and denationalization of French industry, and private education.

Although the antidemocratic aspects of both procedures are widely discussed, scholars also recognize that the package vote and the guillotine can benefit deputies in parliament. This occurs when the government uses the procedures to protect its majority from potentially embarrassing votes on amendments posed by the opposition. Such usage has important stabilizing effects when the government's majority is divided or numerically weak (see Duverger 1987, 98–99; Parodi 1972, 26, 190–93; see also Avril 1965, 447). The procedures can also be useful in limiting or reversing changes made to a bill by the Senate or by a conference committee (*commission mixte paritaire*; see Avril 1971; Duverger 1987, chap. 4; Lascombe 1981).

In short, existing research argues that the guillotine and the package vote are used to protect the government's draft of bills from changes in parliament, to protect deputies in the majority from undesirable votes or debate, and to stabilize fragile government coalitions. These studies fail, however, to explain the conditions that actually lead the government to use the two restrictive procedures. For instance, although the studies suggest that the government uses the procedures to curtail parliamentary changes to government bills, they do not explain what types of bills are most likely to be subjected to the procedures. Similarly, although existing studies state that the procedures can be used to stabilize fragile coalitions, they do not predict how the government will use the procedures when it lacks majority support. We might expect, for example, that use of the guillotine declines during minority government because the government fears censure. We might also expect, however, that use of the guillotine increases during minority government because it may provide the only means for the government to enact bills opposed by parlia-

FIGURE 1

Strategic Committee Proposals under a Closed Rule



Note: x_o is the status quo, x_f is the ideal point of the floor, and x_c is the ideal point of the committee. If $|x - x_o| \le |x_c - x_f|$, then a strategic closed-rule proposal by the committee is $x_f + |x_f - x_o|$.

ment. To fill such gaps in our understanding of the two procedures, we can turn to existing formal models of choice processes.

FORMAL MODELS OF RESTRICTIVE AMENDMENT PROCEDURES AND THE CASE OF FRANCE

Formal models have carefully examined the effects of restrictive amendment procedures on choice processes in the U.S. Congress. Much of this research follows Shepsle's pathbreaking (1979) analysis of structure-induced equilibrium, which demonstrates how policy choices can be understood as a function of the organization of committee systems and of the rules constraining the amendment process. Subsequent models have focused in particular on the importance of committee "gate keeping" power (the power of committees to refuse to report a bill to the floor), "open rules" (where the floor can offer amendments to a committee proposal), and "closed rules" (where the floor must vote either to accept or reject a committee's proposal without the opportunity to amend it).[5]

These models demonstrate that substantial insights into the role of restrictive procedures can be gained from considering strategic behavior by legislative actors.[6] Denzau and Mackay (1981), for instance, show that structure-induced equilibria may not exist when a legislator's preferences on one dimension are linked to preferences on other dimensions. Other models show that legislative rules placing restrictions on the parent body enable strategic committees to bring about outcomes that are not the most preferred policy of the floor (Baron and Ferejohn 1989a; Denzau and Mackay 1983). Under an open rule, a strategic committee will "close the gates" whenever the status quo is more desirable to the committee than is the floor median. Under a closed rule, if the floor's ideal point is between the committee's ideal point and the status quo, a strategic committee can exercise more influence over final policy than would be possible under an open rule. In Figure 1, for example, if preferences are symmetric and single-peaked, then a strategic committee can exercise influence under a closed rule that would be impossible under an open rule whenever the status quo lies in the interval stretching from x_o to x_f (where $|x_o - x_f| \le |x_f - x_c|$). When the status quo is in this interval, the committee can make a proposal in the interval between x_f and x_c that is the same distance from the floor as was the status quo (so that the proposal by the committee is $x_f + |x_f - x_o|$), and this proposal will be adopted (see Denzau and Mackay 1983).

Existing models, then, demonstrate that in the pursuit of optimal policy outcomes, there are incentives for legislators to cut strategic deals, for the composition of majorities to change, and for actors to attempt to alter the dimensions on which tough legislative decisions are made. It is therefore important to understand not only the costs and benefits of strategic behavior, but also how legislators use legislative procedures to limit these costs and to enforce agreements.

Existing formal models of restrictive amendment procedures also shed light on the effects of uncertainty on procedural choice. We are particularly concerned in this study with uncertainty by the legislators about the preferences of the floor. Denzau and Mackay (1983) show that in a unidimensional world where committee members seek to maximize utility (given the committee's probabilistic beliefs about the floor's preferences), regardless of whether the amendment rule is open or closed, sincere behavior is optimal only if the committee believes with certainty that its ideal point coincides with the ideal point of the floor. Therefore, uncertainty introduces inertia into the system because it induces strategic committees to "close the gates" under an open rule. Similarly, uncertainty increases the likelihood that strategic committees will exploit the benefits of a closed rule.

The Case of France

Three differences between congressional and French politics must be considered before existing models of legislative choice can make sense in the French context. The first difference is the "unit of analysis." In Congress, the political actor is the individual legislator, whereas in parliamentary systems the most interesting and important unit of analysis is the political party (see, e.g., Powell 1989; Strom 1990a). In the French case, Converse and Pierce (1986) demonstrate the central role played by political parties in the representation process. The unified nature of party activity is evident in roll call voting analysis, where defections from party discipline are very rare (see Converse and Pierce 1986, 552–62; Wilson and Wiste 1976). Of course, political parties cannot be the only unit of analysis, because the discussion suggests that differences between the government and its parliamentary party (or parties) are sometimes resolved by the government's utilization of the package vote or the guillotine. Hence, the units of analysis in France are parties and the government.

The second difference is the dependence in parliamentary systems of the government on the confidence of parliament. The link between government survival and parliamentary confidence produces two dimensions of preference for each political party: (1) support or opposition for policy and (2) support or opposition for the government.[7] For parties of the opposition, a proposed bill or amendment may represent an improvement over the status quo but nonetheless be rejected to keep clear for voters the distinction between government and opposition. Conversely, government parties in parliament may find a proposal worse than the status quo but nonetheless accept it to remain loyal to the government. The first dimension of choice clearly is tied to the (often Euclidean) spatial assumptions of most existing formal models. The second dimension of choice, however, implies an assumption of dichotomous preference—a party is either for or against the government.

The two dimensions of party utility in parliamentary systems may lead us to different expectations about the use of the guillotine, as opposed to the package vote. When the guillotine is invoked, there is no vote on the actual bill before parliament. Hence, the procedure does not require deputies to take an explicit policy position but, rather, a position regarding support for, or opposition to, the government. The package vote, on the other hand, requires that the deputies take an explicit policy position, because a vote on the bill itself occurs. There is not, however, a need to take a stand for or against the government. The two procedures therefore require choices on different dimensions of party preferences, one dimension related to policy and the other related to support for, or opposition to, the government. If different factors affect the two dimensions of choice, then the conditions leading the government to use the two procedures should not be the same.

The third difference between congressional and parliamentary politics relates to committees and amendment rules. The powers of standing committees in France are not at all similar to those of committees modeled in the existing formal literature. French committees cannot exercise gatekeeping power and are never granted a closed rule, because the government controls the parliamentary agenda (Article 42 of the Constitution). But existing formal models of committee power are applicable to France if we assume that the government is the "committee." Since the government controls the parliamentary agenda, it is endowed with both proposal and gatekeeping powers. In addition, when a government puts its own bill on the agenda, it is the text of the government's bill that serves as the basis for discussion (Article 48 of the Constitution), not the text of the committee's report of the bill.

If we assume that the government fills the role of committees modeled in the existing formal literature, then the "closed rule" in the French context is imposed when the government invokes either of the two "take it or leave it" procedures: the package vote or the guillotine. Of course, it is important to note an inconsistency between the closed rule modeled in the existing literature and the "closed rules" examined in France. The guillotine, unlike a closed rule, requires the National Assembly to choose not between accepting and rejecting a policy, but, rather, between accepting and rejecting the government. And unlike committees in the existing formal literature, the French government selects its own amendment rule.

In short, although there are differences between French legislative institutions and the institutions examined in existing models of legislative choice, the case of France provides an important opportunity to test existing models outside the U.S. Congress.

HYPOTHESES ABOUT THE USE OF RESTRICTIVE PROCEDURES IN FRANCE

I draw on this discussion to develop six specific hypotheses about the conditions that lead the government to invoke the two restrictive procedures. The first three derive from existing formal research on restrictive amendment procedures; the fourth derives from both the formal literature and existing French politics research; the last two derive from existing research on French political institutions.

HYPOTHESIS 1. JURISDICTIONAL COMPLEXITY. *The probability that the government will use restrictive procedures on a bill increases as the number of issue dimensions pertaining to the bill increases.*

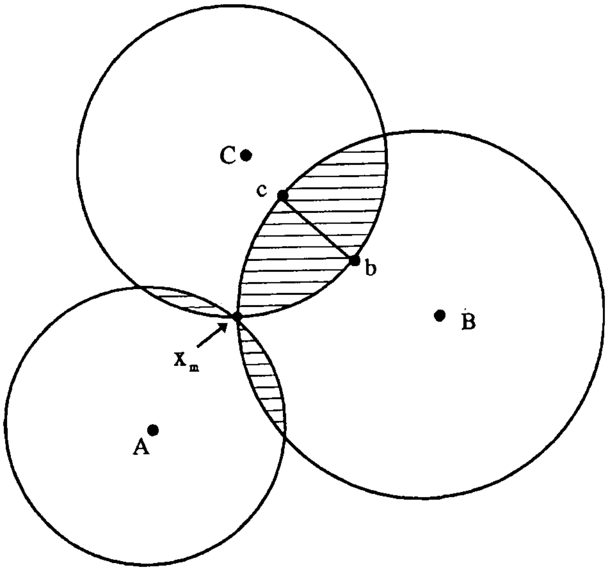
HYPOTHESIS 2. DISTRIBUTIVE BILLS. *The probability that the government will use restrictive procedures on a bill increases when the bill is distributive in nature.*

Hypotheses 1 and 2, discussed in detail by Krehbiel (1991; see also Denzau, Riker, and Shepsle 1985; Shepsle and Weingast 1984b), recognize that closed rules preserve "gains from trade" on dimensionally complex issues. Together, these two hypotheses represent a central result from existing formal models. An example of the logic underlying these hypotheses is given in Figure 2. Here, parties A, B, and C must choose a policy on two dimensions. If the two dimensions are voted one at a time under an open rule, the outcome will be x_m, the issue-by-issue median. However, any proposal in the shaded area (the win set of x_m) will defeat x_m. Hence, any two parties could benefit from gains engineered under a closed rule. For example, parties B and C could make a closed-rule proposal on the BC contract curve between the points b and c. Any point on the line between b and is c not only preferred by both B and C to x_m but is also Pareto-optimal in the sense that neither B nor C could be made better off without making the other party worse off. Both the package vote and the guillotine have the institutional structure necessary to preserve gains from trade in the manner suggested by this example.

Although the logic behind Hypotheses 1 and 2 is the same, jurisdictionally complex bills and distributive bills need to be kept conceptually distinct. A jurisdictionally complex bill touches on more than

FIGURE 2

Jurisdictional Complexity and Closed Rules



Note: x_m is the issue-by-issue median. The shaded area is the win + set of x_m.

one dimension of individual (or party) preference. For example, a bill concerning both the amount of money the state transfers to local communities for education and the total number of public education jobs paid by the state might touch on one dimension of choice related to the decentralization of political decision making and another dimension of choice regarding the amount of state money spent on education. A distributive bill, on the other hand, is one where the benefits are concentrated and the costs are spread out. Following the previous example, a distributive bill might be one where parliament must decide how geographically to distribute a fixed amount of state funds earmarked for education.

Students of parliamentary politics may be skeptical of Hypotheses 1 and 2 since parliaments clearly do not practice distributive politics like that in Congress. Although I do not want to suggest that French parliamentary politics is "distributive" in the U.S. congressional sense, it is important to recognize that parliaments must vote publicly on the distribution of limited public resources. Hence, although individual deputies may not frequently fight successfully on the floor of parliament for particularistic benefits, intuitions underlying Hypotheses 1 and 2 are relevant to parliamentary systems because they can help us to understand how governments use restrictive procedures to address problems between factions within a government party, between parties who are partners in a government coalition, and between the government and opposition parties during minority government.

HYPOTHESIS 3: GOVERNMENT STATUS. *As the government's status becomes weaker or more heterogeneous, the probability that restrictive procedures will be invoked on a bill increases.*

Hypothesis 3 is the only hypothesis strictly related to systemic rather than bill-specific factors. As I have noted, research on French institutions suggests that use of restrictive procedures is related to the government's status but does not provide a satisfying theoretical explanation of the relationship between systemic factors and the use of restrictive procedures. Existing formal research can help fill this theoretical void. In particular, formal research on the effects of uncertainty, strategic behavior, and dimensional complexity all lead to the development of Hypothesis 3.

Existing models show that as uncertainty about the preferences of the floor increases, the benefits to the proposer under a closed rule also increase. Hence, if we assume that a government is more uncertain about the preferences of the majority when the government coalition becomes more heterogeneous or loses its majority status, then closed rules should be used in such situations to combat uncertainty. Existing models also show that strategic amendment activity permits political actors to achieve more desirable policy outcomes than does sincere behavior. In parliamentary democracies, strategic behavior should increase during minority government because a wider variety of strategic coalitions are possible than is the case during majority government, when the roles of government and opposition parties are clearly delineated. Hence, restrictive procedures should be used by minority governments to combat strategic behavior by the opposition.

We can be more confident that the logic underlying formal models leads to Hypothesis 3 if there exists evidence of an increase in strategic voting behavior during minority government. Measuring strategic behavior is problematic, since it requires information about party preferences; but one possible approach in France is to assume that it is rarely sincere for the preferences of the Communists, a left-wing party, at once to differ from the Socialists and be the same as those of the conservative parties on the Right. This assumption, which is consistent with studies of the ideology of French parties and French party supporters (Converse and Pierce 1986), enables us to make inferences about strategic behavior from the voting records of the parties. Evidence of strategic behavior by the opposition parties exists if the Communists and the parties on the Right vote together to defeat the Socialists on motions, articles, or amendments during minority government (i.e., on votes that precede the final vote on a bill) but fail to defeat the Socialists in final votes on bills. Table 1 shows that the Socialists were defeated on motions, articles, and amendments 6% of the time in the first two years of minority government. As a point of reference, the first row of the table shows that this is substantially more often than was true during the Socialist major-

TABLE 1

Strategic Voting during Minority Government

TYPE OF GOVERNMENT	PERCENTAGE OF VOTES WON BY SOCIALISTS: AMENDMENTS, MOTIONS, ARTICLES	FINAL BILLS
Majority	99.2	100
Minority	94.0	99.5

Note: Data in the table are from over 500 votes randomly selected by me. The first row of the table covers the 1981–86 Socialist majority government. The second row of the table covers the 1988–June 1990 period of Socialist minority government.

ity government of 1981–86, which lost similar votes less than 1% of the time. However, in final votes on bills, the Communists and the parties on the Right joined together to defeat only one Socialist bill in over two hundred. Hence, the Communists and the conservative parties voted together only in votes that preceded the final decision on bills. The data in Table 1, then, suggest that minority government encourages strategic behavior by opposition parties; and we might expect the government to use restrictive procedures to combat this strategic behavior.

A final link between formal models and Hypothesis 3 derives from Budge and Fairlie's argument that parties in parliamentary systems are often linked to particular issues; that is, parties frequently "stress particular issues because they almost always work in their favor—they do, in a real sense 'own' them" (1983, 271; see also Budge, Robertson, and Hearl 1987). Parties on the Right, for example, can never hope to appear more committed to social reforms than parties on the Left; and parties on the Left can never hope to appear more committed to military defense than parties on the Right. If individual parties are primarily concerned with different dimensions of policy, then jurisdictional complexity increases with the number of parties in a government coalition. Hence, following Hypothesis 1, restrictive procedures can be used to preserve agreements between coalition parties during multiparty government.

HYPOTHESIS 4: URGENCY. *Restrictive procedures are used to hasten action on the floor of the National Assembly.*

This hypothesis derives from Baron and Ferejohn's (1989a, also 1989b) "divide the dollar" model, which shows that the benefits to all legislators of a closed rule increase as the costs of delay increase. In addition, French politics scholars argue that the procedures can be used to expedite the legislative process (Avril 1965; Duverger 1987, chap. 4). The package vote enables the National Assembly to avoid voting all of the amendments that are introduced by the opposition, while the guillotine immediately stops all debate on a bill.

HYPOTHESIS 5: AVOID POLITICAL EMBARRASSMENT. *The government will invoke restrictive procedures on bills to protect its majority from embarrassing votes and debates.*

This hypothesis cannot be derived from existing formal models because such models assume that voters or parties care only about final policy outcomes. Instead, the hypothesis derives strictly from the research on French politics I have discussed. The arguments presented above dovetail nicely with those of Baumgartner (1987, 1989), who argues that opposition deputies can use the French National Assembly as a tool for turning public opinion against government policies. The hypothesis predicts that as the controversy of an issue increases, pressures by parliament (and specifically the opposition) will increase in forms permitting the maximum focus of public attention on the issue. The government can react to limit the pressure by invoking the restrictive procedures. We should therefore expect the government to use the procedures on the bills that are most vigorously attacked by the opposition and during time periods when public debate on bills is most costly.

Since the logic behind Hypothesis 5 relates to protecting the government majority from undesirable votes or debate, it is difficult to generate different predictions for the two restrictive procedures. The package vote does not protect the majority from opposition attacks because under the procedure, authors can defend their amendments. It does, however, protect the majority from voting these amendments. The guillotine protects the majority not only from voting amendments but also from voting a bill itself. In addition, it protects the majority from protracted debate since all debate ceases immediately when the procedure is invoked. Under the guillotine, however, the opposition has wide latitude to criticize the majority during the debate on the censure motion that generally follows the use of the procedure.

HYPOTHESIS 6: POLICY DISTANCE. *The government will use restrictive procedures to prevent changes by the National Assembly to the government's draft of a bill; or, the probability that the government will invoke restrictive procedures on a bill increases as the preferences of the government and the National Assembly diverge.*

Hypothesis 6 emerges directly from the literature on French legislative politics, which argues that both types of restrictive procedures are used to protect the government's own bill from changes by a hostile National Assembly. This hypothesis is related to the frequent claim that both procedures (especially the guillotine) are antidemocratic.

THE RESULTS

Tables 2 and 3 give the results of multivariate analyses of Hypotheses 1–6 using data that I collected at the archives of the National Assembly in Paris. The

TABLE 2

Logit Analysis of the Package Vote

EXPLANATORY VARIABLES	(1)	(2)	(3)	(4)
Constant	−5.18 (1.12)	−4.38 (.98)	−5.31 (1.12)	−4.45 (.98)
Multiple referrals (*jurisdiction*)	.56 (.78) *.046*	2.38 (.60)	—	1.33 (.72)
Finance Committee (*distributive*)	2.73 (.84) *.46*	—	3.10 (.67)	2.20 (.76)
Coalition government	1.92 (.82) *.27*	2.07 (.79)	1.95 (.82)	1.65 (.78)
Minority government	5.75 (1.48) *.89*	5.39 (1.39)	5.80 (1.47)	5.42 (1.48)
Days until end of session	.002 (.006) *.00013*	.004 (.006)	.002 (.006)	.004 (.006)
Opposition activity (*controversy*)	1.25 (.29) *.14*	1.01 (.26)	1.29 (.28)	1.30 (.28)
Days until next election	−.003 (.001) *−.00023*	−.003 (.001)	−.003 (.001)	−.003 (.001)
Number of adopted amendments	.017 (.007) *.0011*	.011 (.006)	.018 (.006)	—
Report lag	−.01 (.01) *−.00073*	−.01 (.01)	−.01 (.01)	−.01 (.01)
Log likelihood	−43.61	−49.53	−43.87	−46.60
Number of cases	356	356	356	356

Note: The dependent variable takes the value 1 if the package vote is used, 0 otherwise. Standard errors are given in parentheses. The italicized number in column 1 gives the change in the probability of observing the package vote given a one-unit increase in the independent variable when the prior probability of observing a package vote is .07 (7% of the bills in the sample are subjected to the package vote).

TABLE 3

Logit Analysis of the Guillotine

EXPLANATORY VARIABLES	(1)	(2)	(3)	(4)
Constant	−4.00 (1.05)	−3.66 (1.00)	−3.88 (1.03)	−4.14 (1.05)
Multiple referrals (*jurisdiction*)	−.64 (.77) *−.023*	.65 (.61)	—	−.77 (.73)
Finance Committee (*distributive*)	1.97 (.68) *.22*	—	1.63 (.55)	2.02 (.67)
Coalition government	.56 (.76) *.034*	.77 (.73)	.54 (.75)	.64 (.76)
Minority government	1.93 (.86) *.22*	1.67 (.84)	1.89 (.86)	2.01 (.84)
Days until end of session	−.017 (.010) *−.00078*	−.015 (.010)	−.017 (.010)	−.017 (.010)
Opposition activity (*controversy*)	.76 (.29) *.051*	.62 (.25)	.69 (.25)	.74 (.27)
Days until next election	.0001 (.0008) *.0000033*	.0003 (.0008)	.00008 (.0008)	.0001 (.0008)
Number of adopted amendments	−.005 (.008) *−.00023*	−.008 (.008)	−.007 (.008)	—
Report lag	−.007 (1.06) *−.00032*	−.005 (.006)	−.006 (.006)	−.007 (.006)
Log likelihood	−53.17	−57.26	−53.52	−53.36
Number of cases	356	356	356	356

Note: The dependent variable takes the value 1 if the guillotine is used, 0 otherwise. Standard errors are given in parentheses. The italicized number in column 1 gives the change in the probability of observing the guillotine given a one-unit increase in the independent variable when the prior probability of observing a guillotine is .05 (5% of the bills in the sample are subjected to the guillotine).

data set includes information about amendment and procedural activity for 356 *projets de loi* (government bills) that were voted in first reading between the 21 March 1978 elections and the end of the legislative session in December 1989.[8] The bill is the unit of analysis, and the dependent variable takes the value one if the restrictive procedure is used and zero if it is not. The dichotomous nature of the dependent variable makes logit an appropriate model. Of the 356 bills, 37 (just over 10%) were subjected to one or both of the restrictive procedures. The frequency of French restrictive procedures is therefore greater in the restricted sample than the frequency of closed rules in Congress (1975–86), although a larger percentage of bills in Congress are subjected to modified restrictive rules (Bach and Smith 1988, 57).

I test Hypothesis 1 (that use of the guillotine should increase with a bill's jurisdictional complexity) by assuming that bills considered by more than one committee are jurisdictionally complex. Hence, *jurisdiction* is a dummy variable that takes the value one if the bill is referred to more than one committee, zero otherwise. I test Hypothesis 2 by assuming that Finance Committee bills are distributive, because they involve the levying of taxes and the distribution of money. *Distributive*, then, is a dummy variable that takes the value one if the bill's original jurisdiction is the Finance Committee, zero otherwise. The Finance Committee had original jurisdiction for 79 of the 356 bills in the sample, of which 12 were annual budget bills. Both *jurisdiction* and *distributive* should have positive coefficients if Hypotheses 1 and 2 are true.

The effects of government status (Hypothesis 3) are measured using two dummy variables. The first, *coalition,* takes the value one if the government is a coalition majority, zero otherwise. The second, *minority,* takes the value one if the government is a minority government, zero otherwise. The coefficients for each of these two variables are used to estimate the difference between the probability that a coalition or minority government will invoke a restrictive procedure and the probability that a single-party majority government will invoke a restrictive procedure.

I examine whether there is an increase in the utilization of restrictive procedures as legislative sessions approach their end to test Hypothesis 4 (that restrictive procedures are used to hasten decision making). *End session* therefore simply measures the number of days until the end of the parliamentary session and should have a negative coefficient if Hypothesis 4 is true.

Hypothesis 5 is difficult to test because of the problems inherent in determining the types of issues potentially subjecting the government to embarrassing debate or amendments. I consider two plausible approaches. The first measures the level of controversy raised by a bill as a function of the types of efforts made by the opposition to oppose the bill's adoption. *Controversy* measures four activities that the opposition can undertake to oppose a bill after the bill has come to the floor for discussion. The variable is constructed as follows: (1) If the number of amendments posed by the opposition is more than one standard deviation above the mean number of opposition amendments in the data set, the value one is added to the index; (2) if the number of suspensions of debate demanded by the opposition is more that one standard deviation above the mean number of suspensions demanded by the opposition in the data set, then the value one is added to the index;[9] (3) if the opposition demands a verification of the quorum, the value one is added to the index; and (4) if the opposition submits one (or more) of three preliminary motions (moving the previous question, moving the bill inadmissible, moving it be returned to committee for reconsideration) the value one is added to the index.[10] *Controversy* can take values of zero to four, with a higher value indicating a larger degree of controversy. If potential government embarrassment increases with the controversy of the measure, then *controversy* should have a positive coefficient.

Another measure of the potential embarrassment that a bill might cause the government relates to the timing of the bill with respect to the next election. If the government invokes restrictive procedures to avoid electorally difficult votes or debate, then the payoff of invoking the restrictive procedures should increase as elections approach. *Next election* measures the number of days from the time a bill is voted to the next parliamentary or presidential election and hence should have a negative coefficient to support Hypothesis 5.

I test Hypothesis 6 (that restrictive procedures are used by the government to protect its bill from change) by assuming that successful amendment activity is a proxy for differences between government and floor preferences. As the changes to the government's text of a bill increase, policy divergence increases. *Amendments* is simply the sum of all accepted amendments on a bill.

The final variable in the model, *report,* is a control variable not directly related to any of the six hypotheses. This variable measures the number of days between the submission of the bill by the government and the report by the relevant committee. It is a proxy for the importance of the bill to the government, because the government determines when bills are reported and debated in the National Assembly (and hence can call for its most important bills quickly while leaving less important ones buried in committee). *Report* should have a negative coefficient if the probability of restrictive procedures increases with the importance of the bill to the government.

Tables 2 and 3 give four specifications of the statistical model analyzing the government's decision to invoke the package vote and the guillotine, respectively. Column 1 in both tables includes all the independent variables, and the italicized number in column 1 gives the expected increase or decrease in the probability of observing the package vote or the guillotine, given a one-unit increase in each independent variable. The italicized number is calculated based on the assumption that the prior probability of observing the package vote is .07 (since 7% of the bills in the sample are subjected to the package vote) and that the prior probability of observing the guillotine is .05 (since 5% of the bills in the sample are subjected to the guillotine).

First consider Hypotheses 1 and 2. For the package vote, all specifications in Table 2 support Hypothesis 2. Column 1 shows that if a bill is submitted to the Finance Committee, the probability that the bill will be subjected to the package vote increases by 46%. Hypothesis 1 also receives some support in the model, with *jurisdiction* having the expected sign, albeit with a coefficient substantially smaller than that of *distributive* and with a standard error that leaves doubt about its statistical significance. However, an examination of columns 2 and 3 shows that the poor performance of *jurisdiction* in column 1 is probably due to multicollinearity between *jurisdiction* and *distributive.* In column 2, *distributive* is suppressed and *jurisdiction* is highly statistically significant with a coefficient substantially larger than is the case when *distributive* is included. In column 3, *jurisdiction* is suppressed and the coefficient of *distributive* is even larger than is the case in column 1. Although the data do not permit us to sort out the independent effects of jurisdictional complexity and distributive consequences, the specifications in columns 2 and 3 do give us reason to believe that both variables are important to the government's package vote decision. This interpretation is supported by simply noting that 24% of jurisdictionally complex bills in the sample are subjected to the package vote, whereas only 3% of bills that are not jurisdictionally complex are subjected to the package vote.

The guillotine data do not support Hypotheses 1 and 2 to the extent that is true in the case of the package vote. Table 3 shows that *distributive* is statistically significant with the expected sign, supporting Hypothesis 2 but that the effect is much smaller in magnitude than is the case for the package vote. The data for the guillotine, on the other hand, do not support Hypothesis 1. The parameter estimate for *jurisdiction* is not statistically significant (and has the wrong sign) in all specifications of the model, including columns 2 and 3, which attempt to examine the effect of multicollinearity between *jurisdiction* and *distributive* on the parameter estimates for these two variables. In sum, two key hypotheses from existing formal models of closed rules (Hypothesis 1 regarding jurisdictional complexity and Hypothesis 2 regarding the distributive characteristics of bills) are supported by the tests for the package vote. The same is not true for the guillotine.

Next consider Hypothesis 3. The parameter estimates for the variables related to the status of the government support the hypothesis in the case of the package vote. The effect of minority government on the probability of the package vote is very large: if the prior probability of observing a package vote is .07 under a single-party majority government, the probability is .96 under a minority government. The existence of a coalition government also increases the probability of the package vote, but to a lesser extent than is the case with minority government. The existence of minority government also has a large and statistically significant effect on the government's decision to use the guillotine; but Table 3 shows that the existence of coalition government does not have a significant impact on the government's decision calculus.

The results pertaining to minority government deserve special comment because they are significant to our understanding of French politics in two respects. First, Wright (1989), Andrews (1982), and Frears (1981) each argue that parliamentary impotence in the Fifth Republic is due largely to the existence of a progovernmental majority; and they predict that this impotence may disappear in the absence of such a majority. The data presented indicate that this might not be true, because during minority government, the government's widespread use of restrictive procedures can limit the legislative role of parliamentary parties. Second, the government's frequent resort to the guillotine during minority government may have positive implications for cabinet stability. The guillotine enables opposition parties to oppose bills and then abstain on Article 49.3 censure motions. Hence, opposition parties can avoid both bringing down the government and paralyzing the legislative process without supporting either the government or its policies. The fact that French institutional arrangements create this strategy for opposition parties should make minority government in France more stable than might otherwise be possible.

The hypothesis that restrictive procedures are used to expedite the legislative process (Hypothesis 4) is tested by *end session*. Columns 1–4 indicate that the government's propensity to utilize the package vote is not affected by the government's desire to expedite the legislative process. Similarly, Table 3 shows that the guillotine is not used with increasing frequency as the legislative session draws to an end. These results are perhaps not surprising, given uncertainty in existing research about the extent to which the procedures can actually be used to hasten the process. The package vote does not limit the right of authors either to introduce or to defend amendments (Carcassonne 1988, 336), and the guillotine entails the delays of a censure motion.

With respect to Hypothesis 5, the coefficients for *controversy* are somewhat difficult to interpret given the nature of the variables; but their effects are clearly significant for both the package vote and the guillotine. In the case of the package vote, when the value of *controversy* is four (its maximum value), the probability of the package vote is .92 (.07 + .85). Hence, the effect of full-fledged opposition efforts to draw attention to a bill on the probability of the package vote are very important. The same is true for the guillotine, although the magnitude of the effect is smaller.

The number of days until the next election is also used to test Hypothesis 5. The variable is statistically significant in the expected direction for the package vote, although the magnitude of the effect is small. In the case of the guillotine, the coefficient is not statistically significant and has the wrong sign. The direction of the effect may suggest that the government does not like to use the guillotine as elections approach since its usage may send a sign that the government is without a majority for its policies. In fact, in the sample data, if there are less than two hundred days until the next election, less than 2.5% of all bills are subjected to the guillotine, while if there are more than five hundred days until the next election, almost 7% of all bills are subjected to the guillotine.

The number of adopted amendments is used to test Hypothesis 6, which states that the use of restrictive procedures should increase as the policy preferences of the government and the National Assembly diverge. Table 2 shows that the amendment variable is statistically significant with the expected sign. To interpret the magnitude of the effect, it is useful to note that if the number of successful amendments on a bill is equal to the mean number of successful amendments in the sample (i.e., 27 amendments adopted), then the probability of the package vote increases by only .03 over the case where no amendments are adopted. However, if the number of successful amendments is exactly one standard deviation above the average number of adopted amendments (i.e., 68 amendments adopted), then the probability of the package vote increases by .13 over the case where there are no amendments.

Hypothesis 6 does not receive support from the data in the case of the guillotine. In Table 3, the parameter estimates for *amendments* are never statistically significant and have the wrong sign. Tables 2 and 3 therefore suggest that the government's use of the package vote is linked to the policy preferences of the legislative actors, but the government's use of the guillotine is not. This interpretation is further supported by the fact that in the sample 44 amendments are reserved, on average, when the government invokes the package vote, but not the guillotine, on a bill, whereas only two amendments are reserved, on average, when the government invokes the guillotine, but not the package vote. The statistical analysis therefore highlights the fact that use of the guillotine touches dimensions of preference related both to government support and policy outcomes, while use of the package vote touches policy preferences almost exclusively.

As a measure of policy divergence between the government and the National Assembly, *amendments* may raise certain objections. Successful amendments by the National Assembly to the government text may not, for example, be a good proxy for the differences between government and National Assembly preferences, because if a bill is multidimensional, a large number of amendments may not entail a large divergence between government and floor preferences. Similarly, large bills may be subjected to more amendments of all types than are small bills. Since the effects of amendment activity may be difficult to interpret, it is desirable to estimate the model without *amendments* to determine whether the inclusion or exclusion of this variable significantly effects the estimation of the other variables. This is done in column 4 of Tables 2 and 3. It is reassuring to note that the coefficients and standard errors of all the variables remain relatively stable for both the package vote and the guillotine when the amendment variable is suppressed.

Finally, the control variable in the model, *report* is not statistically significant for either the package vote or the guillotine. Therefore, if *report* is a good proxy for the importance of the bill to the government, then the importance of the bill does not affect the government's procedural choice when we control for other factors.

CONCLUSION

The analysis presented suggests that the government's use of the package vote *and* the guillotine is associated with the distributive consequences of bills, the level of controversy raised by bills, and the existence of minority government. The analysis also demonstrates that the package vote and the guillotine arise under different circumstances and are not procedurally interchangeable. The government's use of the guillotine (but not of the package vote) seems to decline as elections approach. More importantly, the government's use of the package vote (but not of the guillotine) increases with the jurisdictional complexity of bills, divergences between the policy preferences of the government and the National Assembly, and the existence of coalition government. The package vote, then, appears to be more "antidemocratic" than the guillotine in the sense that as the policy preferences of the government and the National Assembly diverge, the government uses the package vote with increasing frequency to reserve majority and opposition amendments. We might therefore conclude that the package vote is an institution used by the government *against* its own majority in the National Assembly, whereas the guillotine is an institution used by the government to *protect* its majority in the National Assembly from undesirable votes or debate.

The examination of the conditions that lead the government to invoke the two restrictive procedures reveals a similarity between congressional politics and parliamentary politics that is previously ignored by both Congress and comparative scholars. This similarity emerges from considering two results in the empirical analysis. First, the probability of both the package vote and the guillotine increases on controversial bills and on bills sent to the Finance Committee; and the probability of the package vote increases on bills sent to more than one committee. In Congress, controversial bills, bills referred to multiple committees, and bills referred to the "prestige" committees (e.g., Budget, Ways and Means, and Appropriations) are most likely to be subjected to closed or restrictive rules (see Bach and Smith 1988). French restrictive procedures, then, are used on the same sorts of bills that are subjected to restrictive amendments rules in Congress. Second, when we control for the characteristics of bills, the majority status of the government strongly affects the probability with which the government will resort to restrictive procedures. When the government has more than one party, use of the package vote increases; and when the government has only minority support, use of both the package vote and the guillotine increases. Hence, it seems that restrictive procedures in parliamentary systems may be used to preserve agreements *between parties* in the same way that restrictive voting procedures are used to preserve agreements between individual members of Congress.

Together, these two findings suggest a new focus in future studies of parliamentary politics. In the past, studies of legislative politics in parliamentary democracies generally examine the extent to which legislative power is concentrated in the hands of either the parliament or the executive;[11] and studies focusing specifically on parliamentary institutional arrangements often focus on how legislative rules affect the vertical relationship between executives and parliaments.[12] This study suggests that institutional arrangements play important roles in structuring not only legislative–executive relationships but also strategic interactions between parties that are partners in coalitions and between government and opposition parties during minority government. Consequently, it is important to explore more generally how institutions shape legislative strategies and final policy outcomes when issues divide the government and parliament, parties within a government coalition, and (during minority government) the government and the opposition. This analysis suggests that such further research might clearly reveal claims about the antidemocratic nature of institutions like the package vote and the guillotine to be misguided. Instead, restrictive amendment procedures in parliamentary systems, like restrictive amendment procedures in Congress, might very well be found to enhance the efficiency of legislative bargaining processes and the stability of legislative outcomes. Further comparative research on this question is clearly feasible, since procedures like the French guillotine also exist in Germany (Dalton 1989, 59–60), Belgium, Finland, Italy, and the Netherlands (Inter-Parliamentary Union 1986, Table 40).

The analysis presented also has implications for the formal study of legislative institutions. The formal hypotheses related to jurisdictional complexity, distributive consequences, and the nature of the government coalition are all supported by the package vote data, providing new evidence that efforts to model legislative institutions are in fact producing general propositions about choice processes. At the same time, the guillotine data fail to support hypotheses related to jurisdictional complexity and coalition government, raising important questions about the circumstances under which we can apply existing models to parliamentary arenas.

It is not surprising that existing models explain the use of the package vote better than the use of the guillotine. First, the package vote procedure is directly tied to the policy content of bills, whereas the utilization of the guillotine is not; that is, use of the package vote requires the National Assembly to vote either for or against the government's package, whereas use of the guillotine does not require a vote on the bill but, rather (potentially), a vote on a censure motion. Since most existing formal models assume that party utility is strictly a function of short-term outcomes in a well-defined policy space, it seems logical that empirical results for institutions that touch directly on policy preferences (i.e., the package vote) are better explained by existing formal models than are empirical results for institutions such as the guillotine, which touch less clearly on policy preferences.

Second, use of the guillotine carries with it much higher political costs than does use of the package vote. Both procedures can give a public impression that the government lacks a majority for its policy, is internally divided, or is authoritarian against the directly elected representatives of the voters. But the costs of using Article 49.3 are higher because it invites the most "political" act in parliament—a motion of censure. Consequently, use of the procedure receives much greater attention and criticism from the media. Evidence of the higher political costs of the guillotine comes from simply noting that the guillotine is used much less frequently. From 1959 to 1990, the package vote was used 236 times while the guillotine was used 64 times (Maus 1988, 160, 181–82; Secrétariat Général de l'Assemblée Nationale 1988, 1989, 1990). Since existing formal models do not associate costs with the actual utilization of restrictive procedures, it is not surprising that the use of an institution heavy with such costs (e.g., the guillotine) is less empirically consistent with predictions of existing formal models than is the use of an institution for which the costs of utilization are low (e.g., the package vote). The differences between the package vote results and the guillotine results therefore suggest that it might be fruitful for future formal models of parliamentary politics to explore the inclusion of officeholding preferences into the objective functions of parties, as well as to examine the potential political costs associated with using particular types of procedures.

To conclude, my effort to use existing formal models of congressional politics to study the French government's use of restrictive procedures suggests that the emphasis in previous comparative research on the differences between congressional and parliamentary politics has perhaps led us to overlook potentially important similarities in the two types of systems. Specifically, the data suggest that restrictive legislative procedures are used on the same types of issues for the same types of reasons in the French National Assembly and in Congress. Future studies of parliamentary systems could therefore reap benefits by using the intuitions from formal models to study the types of issues that divide parties, and to explore how institutional arrangements are designed to permit competing parties to make and to preserve policy agreements. At the same time, future efforts to develop formal theories about how procedural structures shape the nature and stability of policy outcomes would be enhanced if attempts were made to develop models of noncongressional institutions and conduct additional tests of existing models in noncongressional settings.

Notes

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1. Formal models of coalition formation are an exception (see, esp. Austen-Smith and Banks 1988, 1990; Baron 1991; and Laver and Shepsle 1990). These studies do not, however, examine legislative politics after government formation is complete.

2. This is a literal translation from the French, for which I cannot suggest an appropriate English term.

3. The name *guillotine* does not come from cynical French political observers who see a parallel between the political procedure found in Article 49.3 and another political procedure commonly employed during the eighteenth-century "Reign of Terror." Instead, the name is taken from a close cousin that exists in Great Britain. The British guillotine, however, simply stops all debate on a bill. There is no formal procedure to attach the vote on the bill to a confidence vote (although the British government can state that it will resign if the bill is not adopted).

4. The full text of the letter is reprinted in Maus 1988, p. 183.

5. See Krehbiel 1989 for an excellent review of the formal literature on restrictive legislative institutions. Dion (1992) analyzes the robustness of Shepsle's structure-induced equilibrium.

6. A *sincere* legislative actor proposes, or votes for, his or her most preferred outcome at every stage in the legislative process, whereas a *strategic* legislative actor proposes or votes for alternatives (or amendments) that may not be most preferred at the intermediate stage in order to maximize utility of the final outcome.

7. See Strom 1990b for an excellent discussion of the office-seeking, vote-seeking, and policy-seeking motivations of political parties in parliamentary systems.

8. Several criteria were used to select the bills. First, since the National Assembly seldom plays a large role in foreign affairs, bills originally assigned to either the Defense Committee or to the Foreign Affairs Committee were not included. Second, to avoid the possibility that National Assembly activity reflects efforts to reverse changes made by the Senate, only bills originally submitted to the National Assembly were included. A random sample of the remaining bills were selected; but all finance bills, which dominate the legislative agenda each autumn, were included.

9. Article 58.3 of the Rules of the National Assembly permits the president of a parliamentary group (or his or her designee) to demand at any time a suspension of parliamentary debate for the purpose of a parliamentary group meeting.

10. If a party moves the previous question or moves that a bill is inadmissible (see Rules of the National Assembly, Article 91), a vote on the motion must occur before the individual articles are debated and voted (Article 104.2). The adoption of either type of motion defeats the bill. Parties can also move that bills be returned to committee for further consideration (Article 91.6–8). All three types of motions provide an opportunity for the opposition to criticize a measure publicly before any changes to the bill are made.

11. See, e.g., Lijphart, 1984 and Powell 1982, chap. 4.

12. See, e.g., Loewenberg and Patterson, 1979 and Mezey 1979.

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PROTESTANT THOUGHT AND REPUBLICAN SPIRIT: HOW LUTHER ENCHANTED THE WORLD

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Arguments about the emergence of modern political theory often claim that Protestantism's significance was that it evacuated the political world, that a more properly political ethic took its place, a "disenchanted" one. I shall consider Luther's understanding of biblical history, thoughts on the Christian prince, and view of the "bonds of union" between Christians in order to understand the relationship between the political and spiritual realms. I suggest that even though Luther argues for the separation of the two realms, his political realm is by no means disenchanted. His politics can only be understood in light of his claims about the purview of God the Father and God the Son. "Political vacuum theories," I suggest, misconstrue the relationship between politics and religion in Protestant thought.

It was Rousseau's claim that Christianity produces no "bonds of unions" between human beings (1984, 183). Bonds of union, he thought, must come from a source other than Christian religion if they are to be politically vital and without virulent excess. Christian religion, he thought,

> having no specific connection with the body politic, leaves the law with only the force the law itself possesses, adding nothing to it; and hence one of the chief bonds necessary for holding any particular society together is lacking. Nor is this all: for far from attaching the hearts of citizens to the state, this religion detaches them from it as from all other things of this world; and I know of nothing more contrary to the social spirit. (1984, 182)

Machiavelli, steering only a slightly different course, thought Christianity untenable because it was unable to guide the prince or would-be prince. That the necessities, the verities, of political life might be known required the exposition he would provide. Christianity invariably fails the political actor. Machiavelli would convey a new knowledge:

> Since my intent is to write something useful to whoever understands it, it appeared to me more fitting to go directly to the effectual truth of the thing than to the imagination of it. And many have imagined republics and principalities that have never been seen or known to exist in truth; for it is so far from how one lives to how one should live that he who lets go of what is done for what should be done learns his ruin rather than his preservation. For a man who wants to make a profession of good in all respects must come to ruin among many who are not good. (1985, 61)

Notwithstanding their many differences, Rousseau and Machiavelli accord in their view that a properly republican ethic must replace a politics *entranced* by principles now anathema to it, principles that can only cripple and diminish political life. Christianity is without bonds of union; Christianity cannot guide the prince. Effective political action demands that the verities of politics not be conflated or confused with those of religion—if the latter can be said to exist at all.

While this general argument about properly political action is one of long-standing pedigree, it has a peculiar resonation with the Protestant *theological* claim that the two realms, spiritual and carnal, must be separated that each may do its own work. Indeed, in Rousseau's thought there is a strange mixture of Protestant sensibility and republican spirit, a mixture lacking in Machiavelli's thought—perhaps because the Christianity of which he conceived belonged to the Roman Church. Whether he intended it or not, Rousseau sounds Protestant when he speaks of the true religion (which would complement his vigorous polity) as being, "without temples, altars or rituals, and limited to the inward devotion to the supreme God and the eternal obligations of morality [as it is found in the] pure and simple religion of the Gospel" (1984, 181).

This peculiar alliance of Protestant thought and Republican spirit is worth considering, not merely because it raises questions about Rousseau's ambivalence toward Christianity but also because the supposition has long been in force that Protestant thought *did* evacuate the political world and that a more properly political ethic was established as a consequence of that evacuation. Protestantism disenchanted the world, it removed the binding injunctions of God from the sphere of worldly activity, and left humankind on its own, without divine support. What Rousseau only hoped for—an inward devotion (Protestantism purified, perhaps) supplementing a purely political ethic—is often taken to be a description of the actual relationship between Protestant thought and Republican spirit. And this has given rise to one or another version of what may appropriately be called *political vacuum theories.*

POLITICAL VACUUM THEORIES

Nature abhors a vacuum, and so do political theorists. Political vacuum theories suggest that the political consequences (unintended, to be sure) of Protestantism's insistence upon separating the spiritual and carnal realms was a vacuum of worldly authority in which a vital political ethic could reemerge. The recurring supposition in these readings is that Aristotle was right: to be human is to be a *zoon politikon* (1958, 5). Augustine's (later, the Reformation's) effort to orient the soul toward the Civitas Dei and away from the things of this world which "sway in their temporal instability" (1984, 5) could, for a time, supplant but not ultimately supersede this human verity. Fifteen centuries of bad (political) faith are brought to an end, ironically, by the very success of Luther and the other reformers' anxious struggle to rediscover and proclaim anew, as Augustine had once (1961, 212), the distinction between the carnal and spiritual realms, a distinction which the Roman Church had all but effaced.

As I have said, the supposition of political vacuum theories is that the political reemerges after an overlong hiatus. Wolin, for example, argues that Luther depoliticized theology while Machiavelli detheologized politics (1961, 142–44). Both assaults served to remove the religious obstacles to the reemergence of the political. Where the scholar sympathetic to Weber's (1958) view of history might announce—and perhaps decry—the onset of the world's *disenchantment* in the face of the onslaught, Wolin and other political vacuum theorists are untroubled by this development. One theorist's loss is another's gain. For the political vacuum theorists, disenchantment heralds not loss but rather recovery.

Sabine, while perhaps not as trenchant as Wolin, sees the significance of Protestantism in much the same way: Luther's withdrawal into the religious sphere left the world to its own devices. "Luther's stress upon the pure inwardness of religious experience inculcated an attitude of quietude and quiescence toward worldly power. Religion perhaps gained in spirituality, but the state certainly gained in power" (1937, 363).

Skinner not unlike Sabine, claims that "the main influence of Lutheran political theory in early modern Europe lay in the direction of encouraging and legitimating the emergence of united and absolutist monarchies" (1980, vol. 2, 113). The autonomy of politics, the infelicity of deriving political obligation from God's Word, marks the onset of modern political thought. "Any attempt to excavate the foundations of modern political thought needs to begin with the recovery and translation of Aristotle's *Politics*, and the consequent re-emergence of the idea that political philosophy constitutes an independent discipline worthy of study in its own right" (p. 349).

Pocock's arguments are consistent with the political vacuum theory, as well (1973, 148–201; 1975). In his view, Augustine initiates an epoch in which history gives way to eschatology (1975, 33). The events in the City of Man acquire whatever significance they may have in the light of their bearing upon the City of God, in which each individual soul may participate. Public history—the secular events that have shaped a people over time—is subordinated to the private search for redemption. Here is the "separation of redemption and history, salvation from society, soul and body" (ibid., 35).[1] Pocock's story is "of the revival in the early modern West of the ancient ideal of *homo politicus* (the *zoon politikon* of Aristotle), who affirms his being and his virtue by the medium of political action" (ibid., 550). While he does not trace the reemergence of a properly political ethic to Luther and the Reformation, he, too, supposes that Christianity (in its Augustinian mode, upon which Luther clearly relied) divested politics of its import to human life.

This *pneumatic* view of Christianity—whether one dates it from Augustine or Luther and the Reformation—has become an axiom on the basis of which theories of the emergence of modern political thought are developed. There is a prima facie case for this view, of course. At first blush it is uncontroversial; even a cursory glance toward Luther reveals his opposition to the Catholic analogical understanding and his insistence (qualified in important ways) that Law and Gospel—the carnal and the spiritual realm, the Old and the New Testaments—are, or pertain to, different orders that must not be conflated.

Luther's insistence upon separating the two realms, however, only captures a portion of what needs to be grasped to understand the subtle relationship between politics and religion in his thought and, by extension, in Protestant thought. The great paradox in Luther (his distinctions notwithstanding) is that the world is *not* disenchanted. A purely political ethic (and here, of course, Aristotle, whom Luther detested [1961a, 470–71] is the counterpoint) could not be countenanced. This is the theoretical perplexity that any interpretation of Luther must address. Luther's pronouncements of the stark disjunction between the two worlds must be juxtaposed with his view that *this* world is not emancipated from the other. Luther does, it is true, adduce a theological argument for separating the two worlds that each may be granted what it is due. This world, however, does not become disenchanted as a consequence of separating religious and worldly affairs. Needless to say, the theoretical significance of the underlying unity of these seemingly opposed notions bears on the question of the disenchantment of the world that Protestantism purportedly occasions. It is my contention here that Protestant thought does not disenchant the Roman Church–dominated world but, rather, enchants the world in a different manner than does the Roman Church. How this assertion bears on political vacuum theories is apparent and will be considered more fully in the Conclusion.

LUTHER'S UNDERSTANDING OF BIBLICAL HISTORY

It is perhaps not immediately clear why speculation about biblical history is pertinent to political theory; yet Protestant thought—theological and philosophical—has been preoccupied with it right from the beginning. Roughly, analogy is to Aquinas and the Roman Church what the dialectic of history is to Luther and Protestant theology, namely, a way to comprehend the relationship between the orders of reality. The dialectic Luther adduces—the theory of history that comprehends the meaning of Christ's *preservation and annulment* of the Old Law (Matt. 5:17)—is one that political theorists have sadly neglected. Yet *the* Reformation question was about the meaning of Christ's preservation and annulment. Were political theorists to recognize this, the extensive passages in, for example, Hobbes and Locke about the Old and New Testaments could be understood in their proper context, rather than dismissed as archaic or veiled discourse. Hobbes and Locke, like Hegel later on (1948), write of biblical history and the meaning of Christ's fulfillment of the Law not because it is a polite convention that must be observed but because massive political implications follow from the answer one provides (for a fuller discussion, see Mitchell 1991).

Luther's view of this dialectic is complicated; yet it is essential to understand its features if we are to establish the context within which he situates his politics.

For Luther, there are three moments of history, each of which has its corresponding "light" or truth: the Old Testament epoch, before Christ; the New Testament epoch, after the Incarnation; and the moment of the final revelation. The first light is the light of *nature*. By this light the meaning of suffering and injustice in the carnal realm, in the world, remains incomprehensible. Within the moment of history in which this first light shines, suffering and injustice are unsuccessfully treated as matters of offense against God. Here are the Jews.

Right conduct, works, and obedience to the Law, however, fail to recompense. It is by the light of *grace*—the promise of the New Testament—that suffering and injustice become comprehensible for the first time. What is more clearly revealed by this light is that God deplores not the death *of* his people but the death *in* his people (Luther 1961e, 391). The debt of suffering and injustice incurred cannot be discharged unless linked to a inward spiritual journey. By enduring hardships and not opposing the inscrutable will of God, the meaning of suffering and injustice is illuminated.

The light of the Old Testament is supplanted by one that illuminates the mystery of God more fully but not yet completely. Hence the need for a third light, more illuminating still than the lights of nature and grace. At the end of history, by the light of *glory*, the mystery of the God who has only partially revealed himself by his Word is illuminated (Luther 1961b, 202–3). The Word, long silenced by the cessation of prophecy and apostolic writing, gives way to the revelation of God that the Word had only partially, though increasingly, disclosed.

Because of the dialectical character of biblical history, each successive light or moment of history cannot be comprehended by the former. Importantly, however, the successive lights are *portended* by the former. The danger, according to Luther, is that Christians will attempt to comprehend the *new* spiritual truth in terms of the Law, the truth of this (post-Incarnation) moment of history in terms of what came before it. The passages on putting new wine in old bottles (Matt. 9:17; Mark 2:22, John 3:6), as well as Hegel's later injunction that "only spirit comprehends spirit" (1948, 255), reinforce the point Luther tries to make. Representationally, the temptation is for the Christian, like Adam, to hide from God (Gen. 3:7–9). And in this epoch, the "terrors of conscience" animate them, in effect, to hide themselves from their spiritual nature (which is disclosed through *passive* Christian righteousness) and to seek refuge in the *active* righteousness of works (Luther 1961d, 79), the pre-Incarnation way to atone for sin:

> Man's weakness and misery is so great, that in the terrors of conscience and the danger of death, we behold nothing else but our works, our worthiness and the law. . . . Thus man's reason cannot restrain itself from the sight and beholding of this active reason or working righteousness . . . nor lift up her eyes to the beholding of passive or Christian righteousness. (1961c, 101)

This picture is complicated, however, by the fact that while Christians attain a foothold in the spiritual realm through Christ in this epoch, they do not become *as God*. Christians retain a "remnant of sin" because they still possess a carnal body. The finite human being remains finite despite the righteousness attained through Christ. In Paul's words, "I see another law in my members, warring against the law of my mind, and bringing me into captivity to the law of sin which is in my members" (Rom. 7:23). The second, penultimate spiritual moment, consequently, remains shrouded in mystery and sin despite the depth of soul attained by the Christian. Christ and the truth of the New Testament deepen the Christian,[2] but fundamental mystery still abounds. The guide, in the midst of this mystery, can only be the Word, the meaning of which will be fully disclosed only at the time of the final Revelation.

TEMPORAL AUTHORITY AND THE ENCHANTMENT OF POLITICS

The political implication of the denial that new wine can be put in old bottles, that post-Incarnation spirit can be comprehended by pre-Incarnation Law, is that temporal authority should not to be allowed to judge the "secret, hidden, spiritual matter" of faith (Luther 1961e, 385). The physical power that is the medium of temporal authority cannot in principle, and ought not in practice, be used to confront the believer. The physical power of earthly kings is of a different order from the spiritual power Christians "possess" through Christ. Heresy, consequently, can never be prevented by force (p. 389). Worldly government extends to life and property, but over the soul it holds no sway; belief and unbelief are matters of conscience, not power (p. 385).[3]

That Luther (and Locke) should implore that the two realms be separated already implies that they have been confused, mixed together in a way that denies precisely what Christ announced: a separation of the carnal and spiritual realms.[4] The carnal and the spiritual realms have been so confused by humankind, Luther says, that bishops rule over cities while lords rule over the human souls (ibid., 389). The religious task is to separate the two realms so that salvation may be achieved—from which it must be concluded that the justification for separating the political and theological realms is theological, *not* political.[5]

Care must be exercised here, however; for while the post-Incarnation spiritual domain of God the Son is "new wine," the "old bottle" of politics that cannot hold it is despiritualized but not desanctified (as political vacuum theories would have it). In Luther's terms, while the government that rules over Christian souls is an invisible government where "all are alike subject to each other" (1961e, 391), nevertheless, because the great majority of human beings will never be Christian, the invisible government that ministers to the piety of Christian souls must be supplemented by a visible government that produces peace. And this visible government is justified not on "purely" political grounds but, rather, on the basis of God the Father's pre-Incarnation covenant with the children of Israel. There is a need for the *temporal* sword for the children of the world, but this is decidedly not a *secular* sword. Hobbes, of course, would later make much of the Mosaic covenant with God the Father in the *Leviathan*, Parts 2 and 4, as well as in portions of *De Cive*, in order to make precisely this point: the real foundation of government is established in the paradigmatic act of the Mosaic covenant linking the citizens to God the Father through an awful Moses/Leviathan.

Returning to Luther, in biblical terms, Moses' Law was given to the children of this world that there might be order; and this sword is not abolished by Christ. Moses' Law still applies to the unrighteous. The fulfillment of the Law by the Gospel purported to be offered by Matthew's Christ (5:17) pertains to Christians only. The sword, therefore, is necessary for the unrighteous and justified by "divine ordinance" (1961e, 411). Indeed, the wrath of the Father, acting through the temporal prince, is necessary to constrain the Nietzschean beast within. "If anyone attempted to rule the world by the Gospel and abolish all temporal law and sword, . . . he would be loosing the ropes and chains of the wild savage beast and letting them bite and mangle everyone" (p. 370).

Were we to stop here it could be said already that Luther did not wish a wholly disenchanted politics in order to attend more fully to the spiritual realm. The Law is superseded for Luther, but he does not completely sever the link between "spirit" and worldly government. Rather, worldly government is given a meaning other than that adduced by the Roman Church. It is comprehended as a moment within the purview of God the Father—a moment that is part of the higher unity of the Trinity yet different from God the Son, who occasions the separation of the spiritual and carnal realms. Worldly government is for the unrighteous. Following Augustine, only true Christians are citizens of the Heavenly City. The post-Incarnation truth is for the few; but because the few must live with the rest, the pre-Incarnation justification of the right to rule granted by God the Father still stands. All this, I should add, reinforces what I suggested earlier—that speculation about the meaning of Christ's fulfillment of the Old Law is political theory disguised in a form that modern commentators often fail to recognize.

I have pointed out why the carnal, earthly realm cannot rule over the spiritual realm: it cannot comprehend it (new wine cannot flow into old bottles). Against the Roman Church, however, Luther argues that the obverse is equally untenable: where the spiritual has dominion over and in the carnal realm, the result is calamitous. This approach, too, would destroy the distinction between the two realms. When this happens, "the door is opened for every kind of knavery" in its name (1961e, 371–72).

> I maintain, that since the temporal authorities are ordained by God to punish evil-doers and to protect the law abiding, so we ought to leave them free to do their work, without let or hindrance everywhere in Christian countries, and without partiality, whether for pope, bishops, pastors, monks, nuns or anyone else. (1961a, 410)

The work of temporal authorities is one thing; that of the spiritual kingdom, another. By leaving temporal authorities free to take care of their affairs (again, this is justified on the basis of the right to rule established in the Old Testament), obstacles to salvation are removed. By insisting that the Church have no say in temporal matters, the work of this world is left to those most able *and authorized* to perform it. Separate the two realms and both operate without doing violence to the other. The possession and use of power in the realm of politics is justified by the Old Testament, and the spiritual power that fulfills the Old is established by the New. God the Father comprehends power, and God the Son comprehends spirit.

On the basis of this argument for separating the two realms, it should be evident that living under the jurisdiction of God the Father is clearly different from living under God the Son. Living under the Father, however, is decidedly not the same as living in a disenchanted world. What political theorists have failed to grasp is that the distinction between enchantment and disenchantment does not map onto Luther's distinction between carnal and spiritual, between God the Father and God the Son. The carnal realm is not spiritual, to be sure (though it portends it); but the former is nevertheless comprehended by a moment of the triune God.

This point is helpful, if not crucial; but it does not take us as far as we need to go. Worldly government, we have seen, finds its authorization in God the Father. Here is a justification for rule over the unrighteous. But what of the righteous, of Christians? What are their bonds of union? These are questions that can be best answered, I think, by looking first at the Christian prince (the person who is both Christian *and* prince, the person who acts *in* the carnal realm but is not *of* it) and then considering the bonds of union between the righteous (Christians), who are not compelled to occupy the limit position that the Christian prince must.

THE CHRISTIAN PRINCE

The prince, as I have mentioned, receives authorization for the use of his power from the Old Testament. This does not mean, however, that all things are permissible, as some German princes erroneously inferred from Luther's stark separation between spiritual and carnal realms (1961e, 365–66). Precisely where the limitations of their power lie will not concern me here. Of greater theoretical significance is Luther's discussion of the *Christian* prince; for in this figure must be found both (political) power and (spiritual) power, which latter at first appears to require an utter rejection of worldly power. The theoretical difficulty is how to be both Christian and prince, how to live in accordance with both the pre- and post-Incarnation dispensations.

When Luther appeals to Christian princes to liberate the German Christians who "are oppressed by distress and affliction" (1961a, 404) he is asking them to exercise power in accord with a spiritual realm uncovered only in the abyss of *powerlessness* and denial of the world. In the Christian prince—that locus of the extraordinary conflation of the two realms—was to be found a release from the yoke German Christendom had borne.

First and foremost, because physical power (of the sort possessed by earthly kings) can only be used to oppose, to stand against, great evil will result if this form of power alone is relied upon. The prince cannot *simply* oppose; such opposition is the way of the world, not the spirit. We must not use our own power to oppose the Church, wrote Luther; for "the greater the power we employ, the greater disaster we suffer, unless we act humbly and in fear of God" (1961a, 406). Humility and fear are, of course, the functional equivalents of the pity Rousseau speaks of in *Emile* (1978, 220–30): they arrest pride before the person whom pride dominates gets lost in the temptations of the world. But power must be exercised, nevertheless; for physical power is needed to oppose the equally physical power exercised by the Roman Church over the German states.

This may seem like a trivial matter to the modern ear, but it was not trivial to Luther; for the prince who relied only on the physical power at *his own* disposal was bound to lose sight of the Source of all power and so suffer hubris, thus foreclosing an encounter with the abyss of Christ, which alone could make him a Christian prince. Weber's definition of political power as "the monopoly of the legitimate use of physical force within a given territory" (1946, 78) may seem axiomatic to us today; but the idea would have seemed ludicrous to Luther. (We tend not even to see it as a theoretical difficulty.) Luther would say that the Weberian view could only countenance hubris and that it is un-Christian—to which Weber would emphatically answer "Yes."

The intractability is resolved by enjoining princes to rely on the power of God, not their own power, to oppose the oppressors: "We must not begin by assuming we possess much strength or wisdom, even if we had all the authority in the world. For God cannot and will not suffer a goodly enterprise to be begun if we trust in our own strength and wisdom. God will surely abase such pride" (Luther 1961a, 405). The power that is to be used *in* the world must come from beyond it;[6] only when the power used to oppose the Roman Church comes from God, in other words, *can* German Christendom be released. The foreign threat can be averted only with God's assistance. A higher power must be the source of worldly power if worldly power is to have a beneficent effect at all. Only with assistance from God can a true defense be made against "the ravening wolves who come dressed in sheep's clothing" (p. 422).

In domestic affairs, too, both power itself and (more importantly) the pattern of its use must come from God, rather than from the prince's person. The Christian prince must act in a manner befitting both an earthly king and a Christian subject. The prince must exercise power, it is true; but the use to which such power is put is informed by the prince's experience as a Christian. He is not powerless toward his subjects, but his servitude before Christ establishes the true nature of his relationship with his own subjects. Christology offers a pattern for polity.[7] The Word, at any rate, offers guidance. Because God once spoke as an ass, for example, the prince must respect people of even the lowest station; but because God's highest angel fell, the prince must trust no one (Luther 1961e, 395). Christian subjects, Luther enjoins, are both Christian and subjects. In their latter capacity they occasionally conspire to acquire the power of earthly kings. Respect and distrust go hand in hand. Here we see the political wariness of Federalist 10 (1961, 77–84) still wearing overtly theological garb. Both are dubious about the beneficence of human motivation—and yet both would carve out a domain in which the self was sacrosanct.

Guidance for political life, I have said, comes from the Word. This does not mean, however, that the actual laws instituted by the Christian princes must be taken verbatim from the Bible. Tocqueville reminds us, after all, that the Gospel enjoins only general principles (1966, 445). The right of making law is justified by the Word of God, but the content of such laws is not to be found in Scripture itself.

Just as new wine cannot be put in old bottles, so, too, the spiritual lesson of Christ that must serve as a guide to the Christian prince cannot be reduced to the letter, to lawful commands. This was Hegel's point in "The Spirit of Christianity and Its Fate" (1948, 209); and it is a crucial insight for the reformers. Reformation political theology could not simply construct a politics drawn from Scripture alone. Strauss completely misunderstood this when he said that had Locke wished to develop a political theory based on the Bible, he would have written a *Politique tirée des propres paroles de l'Écriture Sainte* (like Bossuet) rather than the *Two Treatises* (1974, 205). Locke and the other reformers could not lift chapter and verse from Scripture and make them political laws; for that would have violated the very principle that the Reformers insisted upon, namely, that the spirit and the Law are different yet dialectically related. The laws of the Christian prince, therefore, divulge something of a living tension between the letter of the Law and the spirit that informs it—one that is resolvable only by relying upon God rather than self:

> We should keep written laws subject to reason, whence indeed they have welled as from the spring of justice, and not make the spring dependent on its rivulets, nor make reason captive to the letter.
>
> I know of no law to prescribe for a prince, but will simply instruct him what the attitude of his heart and mind ought to be . . . so that if he governs himself thereby, God will surely grant him the power to carry out all laws . . . in a proper and Godly way. (Luther 1961e, 394, 402).

The laws of the Christian prince, then, are not simply his own. They issue from his person but are guided by God; and in the post-Incarnation epoch such "guidance" should involve a recapitulation in the (domestic) political domain of the pattern established by Christ and all Christians: "Thus a prince should in his heart empty himself of his power and authority, and interest himself in the need of his subjects, dealing with it as though it were his own need. Thus Christ did unto us; and these are the proper works of Christian love" (ibid.).

BETWEEN THE RIGHTEOUS: BONDS OF UNION IN CHRIST

For Luther, Christians cannot have a genuine affiliation with others without the assistance of Christ. Only through a "marriage" between a single Christian and Christ where no others are involved does the basis for human community come to view. At one level, of course, this tentatively confirms what Rousseau would later insist—that the "other" for Christians is Christ, not other citizens—from which no genuine political affiliation could come. But this is not the whole of the matter. Indeed, Rousseau's error stems from not seeing how the pattern of the singular relationship, the marriage, turns back onto the world, so to speak, and offers a pattern for the right relationship between human beings.

In a word, the obligations that bind one human being to another derive from the relationship between the individual Christian and Christ. Through a marriage with Christ that excludes all others, the Christian is able to—and indeed must—include all others; the pattern established by the bridegroom (Christ) is reproduced in the bride's (the Christian's) relationship to all others:

> As our heavenly Father has in Christ freely come to our aid, we also ought freely to help our neighbor through our body and works, and each should become as it were a Christ to the other that we may be Christs to one another and Christ may be the same in all, that is, that we may be truly Christian. (Luther 1961d, 76)

The marriage with Christ, as it turns out, provides the basis for a twin orientation—toward the Divine and toward all others: "A Christian lives not in himself, but in Christ and his neighbor. . . . He lives in Christ through faith, in his neighbor through love. By faith he is caught up beyond himself unto God. By love he descends beneath himself unto his neighbor" (ibid., 80).

The relationship to the neighbor is not based on love alone, however. The righteous Christian has no need for the sword and would endure suffering from it if it came to that. With regard to self, the Christian must be powerless; but with regard to the neighbor, the Christian must punish the enemy who harms. There is no right of revolt (this would come later with Locke), but there is the right to protect: "[Although] you have no need [of the temporal sword and the law] and are not to have them, [you are] to serve those who have not progressed as far as you and still need them. Although you do not need to have your enemy punished, your weak neighbor does" (ibid., 374).

These examples should make clear that the coming to presence of the spiritual realm, the marriage with Christ with which Luther was so concerned, was to *bear fruit* in the (carnal) realm—the central motif of the Gospel of Matthew. It is to be the occasion for reproducing the pattern established by Christ in the relationship with the neighbor. The Christian "labors on earth not for himself but for his neighbor" (ibid., 373). Through Christ the Christian enters the spiritual realm, which occasions both freedom from, and supersession over, the carnal realm; the Christian attains a freedom from all *things*, that is, everything carnal. Here, the Christian reproduces the pattern established by Christ and, like Him, becomes a servant to all. Christians depart from the carnal world to return to it grounded in the spiritual realm that provides the wherewithal for their authentic return. Only "a pattern set up in the heavens" (Plato 1985, 284) makes possible genuine sociality.

CONCLUSION

Consider Rousseau's thoughts on Christian politics in *The Social Contract*:

> But I err in speaking of a Christian Republic; for each of these terms contradicts the other. Christianity preaches only servitude and submission. Its spirit is too favorable to tyranny for tyranny not to take advantage of it. True Christians are made to be slaves; they know it and they hardly care; this life has too little value in their eyes. (1984, 184)

While partially correct on the matter of servitude and submission, he clearly failed to understand that even the seeming otherworldliness of Protestant Christianity, insisting on a separation of the political and religious realm, could provide the bonds of union for a community. Rousseau recognized Protestantism's separation between the two realms but did not recognize the bearing of God the Son (in addition to that of God the Father) upon political life.

Straining Rousseau somewhat, in the idiom I have been using here, the problem he sought to redress was that God the Son had overpowered God the Father, that people had turned to the utterly internal realm of faith (the Son) to the detriment of political life (the Father). This is, of course, ironic; for Luther had argued that while God the Father authorized earthly kings to chastise the unrighteous, genuine community was possible only through God the Son.[8] The bonds of union that Rousseau thought so essential could only come, according to Luther, from the mediator of faith (the Son), who also occasions a City of God *next to* the City of Man. Were Luther to answer Rousseau he might have said that if Christians were only servile and unable to form a viable political community, it was because they understood the path (servitude), but not what it led to (being brothers in Christ). In effect, the problem would be that they were not Christian enough. As Christ was a stumbling block to the Jews (1 Cor. 1:22–23), so was He to Rousseau also.

My intention here, however, is not simply to contrast Luther and Rousseau. It is to suggest that the kind of error Rousseau makes—that of attributing to Protestantism a wholly otherworldly orientation—is the same kind of error made by adherents of one or another version of the political vacuum theory about which I spoke at the outset. Protestantism does not disenchant the political world; the world is the Father's, spirit is the Son's.[9] The republican spirit that Rousseau so hoped to see supplement the "pure and simple religion of the Gospel" (1984, 181) cannot be understood to have diffused into the evacuated political space left in the wake of Protestantism's separation of the political and religious realm. A wholly disenchanted republican politics does not emerge victorious as a result of Protestantism's attempt to return to an uncorrupted Christianity; that is, if the republican spirit came to predominate in certain regions, the mechanism by which it came to do so was not an evacuation of religious content from the political sphere accomplished by Protestant thought. The world remains enchanted in Protestant thought, for different reasons than it had been in Catholic thought, to be sure; but the prima facie case that Protestantism's indictment of the world is equivalent to its desantification does not hold up under scrutiny. For the unrighteous, the world remains under the sway of God the Father; for the righteous few, the world may perhaps for a time be modeled after the pattern established by God the Son.

Notes

1. A perusal of Augustine's *City of God* 5.12–19 (1961, 196–212) suggests that the contrasts Pocock makes are simply too stark. Augustine suggests that Rome itself was necessary in order for salvation through Christ to occur. The biblical passage here is Galatians 4:4 ("But when the fullness of time had come, God sent forth His Son"). Augustine argues—much as Tocqueville would later (1966, 446)—that there was something about Rome's activities in the world that made it possible for Christianity to take hold. *The fullness of time* suggests that at a certain moment the carnal world was able to accommodate spirit. And Augustine thought that the greatness of Rome was that it prepared the way in time for the advent of Christ. His interest was not wholly in turning allegiance away from the City of Man (Rome, literally and symbolically); for it was through Rome, through the carnal realm, that the spiritual realm came to presence. In a word, contra Pocock, the separation between redemption and history, salvation and society, soul and body, is not unequivocal. As Augustine well knew and noted in his *Confessions* (1961, 100), the road to God must pass through Rome (literally and symbolically).

2. Consider Nietzsche on this matter, especially in regard to my earlier comments about the second light of history (the light of grace of Christianity) disclosing the meaning of suffering. "The meaninglessness of suffering," Nietzsche says, "*not* suffering itself, was the curse that lay over man so far—*and the ascetic ideal* [Christianity] *offered man meaning*" (1967, 162). This ascetic ideal "brought fresh suffering with it, deeper, more inward, more poisonous, more life destructive suffering: it placed all suffering under the perspective of *guilt*" (ibid.). This agreement between Luther and Nietzsche about the deepening of humankind accomplished by Christianity may be their only common ground. Nietzsche's contempt for the Reformation, that epoch in which the notion of the equality of all finally triumphed, is all too apparent (ibid., 54).

3. Locke would later make a similar claim in his *Letter Concerning Toleration* (1955, 18). His argument there and in *The Reasonableness of Christianity* (1803) is based on a reading of Christ's fulfillment of the Old Law and in *this* respect covers no new theoretical ground. I have discussed this elsewhere (Mitchell, 1990).

4. Hobbes, of course, was at great pains to deny that there was a spiritual realm at all. Christianity, in a word, did not occasion a separation between two realms. Prior to Christ, as well as after his first coming, there is only the world—the carnal realm (1962, 340).

5. See Smith 1991 for the disturbing implications this has for the constitutional debate about religious freedom. On his view, religious freedom today, "at least as it has come to be understood, forbids governmental reliance upon religious justifications as a basis for public policies or decisions. Therein lies the paradox—our constitutional commitment to religious freedom undermines its own foundation; it cancels itself out by precluding government from recognizing and acting upon the principle justification supporting that commitment" (p. 200). Theologically, the world (politics) cannot stand without a deeper (nonpolitical) foundation.

6. Compare this to Augustine's "When You are our strength we are strong, but when our strength is our own we are weak" (1961, 89).

7. The Leviathan about which Hobbes speaks can be seen as a recapitulation of this pattern. The Leviathan, king of the children of pride, like Christ, takes in upon himself the pride of the children. What Hobbes wanted above all was to quell

human pride; he thought a Leviathan who, like Christ, was a mortal-God (1962, 132), was the only figure capable of doing so. Hobbes's genius and Achilles heel was to have transposed a religious symbol into a temporal one. Instead of a religious mode of atonement for the problem of pride, he offers one that is temporal.

8. Locke would later argue the same point. Prior to Christ and Christian virtue, he asserts, the Law "reached no farther than those things, that would serve to tie men together in subjection" (1803, 225).

9. Most importantly, because the Word was *made flesh* in the Son (John 1:14), those who confess the way of Christ are offered a way—indeed the only genuine way—to form a community. From Augustine to Luther to Bonhoeffer (1955) in the twentieth century, this does not change.

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THE THEODICY OF THE *SECOND DISCOURSE*: THE "PURE STATE OF NATURE" AND ROUSSEAU'S POLITICAL THOUGHT

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Since Rousseau, and perhaps because of him, political theory has often been characterized by a disjunction between considerations of human nature and questions of justice and law. Yet, reexamining the Second Discourse *as a theodicy forces us to rethink how the essential questions of political theory are related just where they seem to become separated. The core of the theodicy is Rousseau's view that we are, by nature, physical beings embedded unproblematically in nature—good, or ordered, beings in a good, or ordered, whole. Although Rousseau presents his explicitly political thought as a juridical doctrine seemingly separated from his understanding of human nature and history, his political thought is founded upon his portrait of man's existence in the "pure state of nature." This portrait serves as a positive formal model to enable us to remake our corrupted existence through the legitimate state—itself modeled by Rousseau on the divine or natural whole.*

As citizens and students of the liberal regimes of modern times, we are heirs to a problematic political project and political science. Modernity, especially as it comes out of the Enlightenment, might be characterized as an undertaking both dedicated to the establishment of liberty and founded upon a reductionist study of humans and their political life. The problematic character of modernity and liberalism is thrust to our attention today as those who have thrown off their shackles look to our political experience and political science for guidance at the same time that they question its substance and sufficiency. Now is the time not for self-satisfaction but self-knowledge.

Jean-Jacques Rousseau faced the dilemmas of modern politics and political science with a clarity seldom rivaled. He was the first important critic of modernity and liberalism within the modern and liberal fold itself. Rousseau is at the font of the peculiar self-questioning character of modernity and perhaps its most profound and influential critic. His acute queries and his troubled gaze have influenced critics of modernity on both left and right, and his questions continue to be our own. In order to begin better to understand our predicament and ourselves, therefore, a serious consideration of the political thought of Rousseau is warranted. I want to begin such a consideration here through an inquiry into the relationship between Rousseau's understanding of human nature and his political theory.

The relationship between Rousseau's discussion of human nature and history in his *Discourse on Inequality*, or *Second Discourse*, and his explicitly political thought as presented especially in his *Social Contract* is a vexed issue in the scholarship of his thought. Why does he write the *Social Contract* as if he had never written the *Second Discourse*? More specifically, why does what he describes as the core of his thought as a whole—the natural goodness of man, and the personal dependence which is the primary cause of our degeneration from good to evil beings—seem to be virtually absent from the most comprehensive presentation of his political thought? This issue can be rephrased to bear on the development of political theory after Rousseau and to address our need to understand how we have arrived where we are today.

Readers of Western political thought since the time of Hobbes have become accustomed to the concept of the *state of nature*. Hobbes portrays the state of nature to make us squarely face ourselves, giving us a correct understanding of our nature and its bearing for the proper erection of the state. Similarly, Locke made the state of nature central to his theory, in particular using it to show the origin of property in appropriation and the need to secure it and ourselves through the formation of a political society. In other words, Hobbes, Locke, and their successors used the state of nature at the same time as a juridical concept and a hermeneutic to explore human nature, two issues that are inextricably related in their thought. Rousseau, however, offers not one state of nature but two. In the part 1 of the *Second Discourse* he describes our original or natural condition through a portrait of savage man's basically solitary existence in the *pure state of nature*. In the part 2 he discusses the *state of nature* simply—the prepolitical condition eventually ended by the social contract. The state of nature in the second sense, shorn of almost everything except its status as a legal concept, is what Rousseau brings to the *Social Contract*, thus splitting his inquiry into human nature from his political theory. The relevance of the pure state of nature and our natural goodness is unclear in Rousseau's thought—a question raised by Voltaire when he quipped that Rousseau's work made him want to walk on all fours, though he regretted that his ill health prevented him from doing so (cited in Rousseau 1959–69, vol. 3, p. 1379).

The bifurcation of questions about human nature and concepts of justice and law apparent in Rousseau's work has infected political thought to this day. This split has been exacerbated by Rousseau's introduction into political thought of the notion of humanity as a historical phenomenon. He summarizes the lesson he would have the reader of the *Second Discourse* take from the "immense space" between the two states of nature he offers: "the human race of one age" is not "the human race of another" (1964, 178). We are historical beings. His pronouncement has radically affected the place of the social contract in political thought in particular and the character of political thought in general. The concept of the state of nature was sublated into the dialectic of history and the history of philosophy or dismissed as a wrong-headed hermeneutic, a quaint or even reactionary chimera, as when Marx wrote: "Let us not go back to a fictitious primordial condition. . . . We proceed from an economic fact *of the present*" (1964, 107). Even with the waning of the historical schools, we are left with an almost universal explicit or implicit acceptance of historicism, a heritage traceable to Rousseau. Where it survives as a concept in political thought, the state of nature remains a mere hypothetical legal starting point, independent of any elaboration of the truth about human nature (e.g., Rawls 1971). In sum, contemporary political theory is generally characterized by a reluctance to inquire into human nature and an emphasis on questions of justice, fairness, procedure, and the like detached from any explicit theory of our nature.

If questions about human nature and such concepts of political theory as justice and law are discussed separately today, the appearance of their separation in the political theory of Rousseau is deceptive. Rousseau's political thought and his constructive philosophy in general are founded upon his understanding of our original nature as seen in his portrait of natural man's existence in the pure state of nature. By examining how Rousseau's legalistic political theory is related to his understanding of human nature, I hope to throw light on how these questions of politics are inseparable from those of human nature at the point in the history of political thought where they seem to become separated. Whatever the validity of Rousseau's own understanding of human nature, he forces us to rethink how the essential questions of political theory are related.

I will argue that the bearing of Rousseau's pure state of nature for his political thought can be seen by examining how he conceives his demonstration that we are naturally "good" to be a justification of nature, or (as he explained his work to Voltaire) a justification of God and his providence (1959–69, vol. 4, p. 1061)—in short, how the *Second Discourse* is intended to be a theodicy. The core of Rousseau's theodicy is his conception that we are by nature physical beings embedded unproblematically in the physical whole of nature. By nature, we are good, or ordered, beings in a good, or ordered, whole. Rousseau's portrait of our original position as good beings in a good natural whole serves (especially in contrast to Hobbes and Locke) as a positive formal model to enable us to remake our corrupted existence through the legitimate state, itself modeled by Rousseau on the divine or natural whole.

ROUSSEAU'S METHOD IN THE *SECOND DISCOURSE*

The announced subject of the *Second Discourse* is the origin and foundations of inequality among men. At the outset of the work Rousseau asks, "How can the source of inequality among men be known unless one begins by knowing men themselves? And how will man manage to see himself as nature formed him, through all the changes that the sequence of time and things must have produced in his original constitution, and to separate what he gets from his own stock from what circumstances and his progress have added to or changed in his primitive state?" (1964, 91). In order to see man as nature formed him, therefore, Rousseau must strip away the changes produced by our "progress." The *Second Discourse* takes the form of a history of mankind from our original condition to our present state.

Rousseau claims that his account in the *Second Discourse* is "conjectural" or "hypothetical." The character of his hypotheses about our original condition and subsequent development is a difficult and contentious subject. Rousseau's intentions regarding the historical status of his account are not, however, relevant for the present discussion because of how his historical and psychological investigations in the work are related. For example, after revealing that the "human race of one age" is not "the human race of another," he explains how the attentive reader will come to this conclusion: "He will explain how the soul and human passions, altering imperceptibly, change their nature so to speak; why our needs and our pleasures change their objects in the long run; why, original man vanishing by degrees, society no longer offers to the eyes of the wise man anything except an assemblage of artificial men and factitious passions which are the work of all these new relations and have no true foundation in nature" (1964, 178). The *Second Discourse* is not essentially or only a historical account, but is most importantly a psychological inquiry into human nature as it may be found universally through proper investigation.[1]

History, for Rousseau, is the successive conditions that decisively shape what the individual comes to be with his fellows. We are historical beings in the sense that the extent and direction of the development of the individual are determined by the extent and character of his relations with his fellows, (i.e., the particular condition into which he is born), especially how those relations and conditions determine the character and extent of his needs, passions, and reason. Rousseau uses a revealing image to elucidate the manner of his search at the outset of the *Second*

Discourse: "Like the statue of Glaucus, which time, sea, and storms had so disfigured that it looked less like a god than a wild beast, the human soul, altered in the bosom of society . . . has, so to speak, changed its appearance to the point of being nearly unrecognizable" (1964, 91). We change "in the bosom of society"; society dramatically (and perhaps irreversibly for the individual) changes what "man" appears to be. We must remove the "encrustations" of society in order to see our true form. Rousseau's description of our original nature and condition and the conditions of our subsequent development is an account not only of what we were and have become but of what we still are. In other words, every child begins life at the point as natural man and would develop into that being apart from the conditions that will actually cause him to develop differently. This is the premise of Rousseau's *Emile*. "Emile is not a savage to be relegated to the desert," Rousseau writes: "He is a savage made to inhabit cities" (1979a, 205). The genetic account of the *Emile* and the historical account of the *Second Discourse* are compatible and complementary (see Starobinski 1964, lv; idem 1988, 12–21).

Rousseau's explanation of how we are historical beings is related to his criticism of his predecessors and indicates the method he uses in his work. He claims that "the philosophers who have examined the foundations of society have all felt the necessity of going back to the state of nature, but none of them has reached it." They have not "found" the state of nature because they have not hypothesized that human nature is actually radically different from what it appears to be: "All of them, finally, speaking of need, avarice, oppressions, desires, and pride, have carried over to the state of nature ideas they had acquired in society: they spoke about savage man and they described civil man" (p. 102). Rousseau claims to be the first to have correctly seen human nature. Speaking of himself in the *Dialogues*, he writes:

> The prejudices that did not subjugate him, the factitious passions to which he was not prey did not hide from his eyes as they did from others those original traits so generally forgotten or misjudged. These traits so novel for us and so true once they are traced could still find, deep in people's hearts, the attestation of their correctness, but they would never have sought them out themselves if the historian of nature had not started by removing the rust that hid them. (1990a, 213–4)

Those who have failed to see themselves, then, have failed to remove the "rust" that has obscured their true nature. They have taken appearance for reality: "They all seek their happiness in appearances, none is concerned about reality. They all place their being in appearance" (*ibid.*).

Rousseau maintains that previous thinkers failed to recognize that "the human race of one age" is not "the human race of another," that "the soul and human passions, altering imperceptibly, change their nature so to speak." By looking at men as we see them around us, we only see individuals that are the product of certain conditions found at a certain time. This is the error made by Hobbes, who claimed that "whosoever looketh into himself . . . he shall thereby read and know, what are the thoughts, and Passions of all other men," so that his state of nature is an "Inference, made from the passions" (*Leviathan*, intro., chap. 13). Despite an apparently similarity of method, however, Rousseau and Hobbes come to drastically different conclusions. In an unpublished writing, the so-called *État de guerre*, he groups his predecessors together, the natural law theorists along with the philosophers of the state of nature, and explains that they all make the same methodological error in their use of the "analytic method." The "error of Hobbes and the philosophers" is to have confounded "natural man with the men that they have before their eyes, and of transporting into one system a being that can subsist only in another" (1959–69, vol. 3, p. 611–12). As Rousseau warned a critic of the *Second Discourse*, "Men differ so much according to times and places that . . . inferences from the particular to the universal are liable to lead to quite contradictory and inconclusive conclusions" (p. 235).

Through his own portrait of savage man's solitary existence in the pure state of nature, then, Rousseau recounts the conditions that will reveal our fundamental nature. His own inquiry into the various causes that shape human nature leads him even so far as to wonder whether any of the faculties taken by his predecessors to be distinctively human are in fact constitutive of mankind.

PHYSICAL MAN AND MORAL MAN

Rousseau claims in the *Second Discourse* that "the philosophers who have examined the foundations of society have all felt the necessity of going back to the state of nature." However, it is not true that "all" the philosophers have felt the need to investigate the state of nature in this way, or even at all. The scholastics and later natural law theorists and jurists, for example, considered the state of nature and even pure state of nature, but not as a necessity in explaining the foundations of society. Only with Hobbes and Locke, most prominently, could such a method be said to have become obligatory for understanding human nature and politics. Rousseau therefore indicates that his true antecedents are Hobbes and Locke, while at the same time, by referring to both the state of nature and pure state of nature, he evokes his more traditional predecessors. Rousseau's transformation of the the terms of the natural law theorists points to how he will challenge the understanding of nature and human nature implicit in perhaps all previous views of human nature.

Aquinas and Suarez—later, Hooker, Grotius, and Pufendorf, among others—all considered the state of nature in one way or another. They conceived of that state as the human condition prior to the Fall (the state of innocence, as opposed to the state of sin and to the state of grace or redemption). They also sometimes entertained, in addition, a wholly speculative and hypothetical pure state of nature, or the state of nature without or prior to divine dispensation. Neither this strictly conjectural pure state of nature nor the lost state of nature as conceived by these thinkers could provide much practical guidance for political society, precisely because these conditions were viewed as either hypothetical (i.e., conflicting with the scriptural account) or irretrievably lost because of man's Fall. The principal role of the hypotheses about these two conditions in these theories, then, was to elucidate the character of the universally available and applicable natural law.[2]

Rousseau indicates his awareness of the background of the concept of the state of nature when he writes, "It did not even enter the minds of most of our philosophers to doubt that the state of nature had existed, even though it is evident from reading the Holy Scripture that the first man . . . was not himself in that state; and that giving the writings of Moses the credence that any Christian philosopher owes them, it must be denied that even before the flood men were ever in the pure state of nature." His complaint, however, is not that the philosophers of the state of nature have proceeded in supposed ignorance of or opposition to Scripture, but that they have not pursued their inquiries far enough in that direction: "They spoke about savage man and they described civil man" (1964, 102–3).

Unlike those who first discussed the state of nature and pure state of nature, Rousseau's pure state of nature is not the condition without or prior to divine dispensation but without or prior to any truly social or moral relations among humans. The pure state of nature in the *Second Discourse* is the condition where humans exist as purely physical beings, that is, possessed solely of the instincts, passions, and faculties with which they are endowed by (physical) nature. Humans originally or naturally lack moral needs or passions, that is, needs or passions with particular, conscious regard for their fellow humans. Such needs and passions, Rousseau argues, only develop when individuals begin to have constant relations with their fellow humans. Rousseau defines a moral being as "intelligent, free, and considered in its relations with other beings" (1964, 94). The natural man whom he describes in the pure state of nature is manifestly not a moral being according to his definition: while he may be said to be "free" in the sense that he enjoys "natural freedom" (p. 127)—or what Rousseau equates with "an unlimited right to everything that tempts him and that he can get" (*Social Contract* 1.8), he is not "intelligent" nor does he have "relations with other beings" or the passions associated with such relations.

The distinction between the physical and the moral in general is intimated by Rousseau at the outset of the *Second Discourse* in a way that presages the structure of the work as a whole: "I conceive of two sorts of inequality in the human species: one, which I call natural, or physical, because it is established by nature and consists in the difference of ages, health, bodily strength, and qualities of mind or soul; the other, which may be called moral or political inequality, because it depends upon a sort of convention and is established, or at least authorized, by the consent of men" (1964, 101). The natural, or physical, being of man consists of attributes "established by nature" in the individual, including both bodily and psychological attributes. In part 1 Rousseau speaks of man in the pure state of nature as a physical being with solely the endowments of physical nature. The moral, in turn, seems to be divided into two sorts: that which is moral in the sense "established" by "a sort of convention" as men begin to associate together and another, more "political" form that is moral in the sense of being "authorized, by the consent of man." In part 2 Rousseau shows individuals coming together and becoming moral beings in the first sense, eventually to become political beings through the convention of the social contract (hence moral in the second sense; see *Social Contract* 1.8). The distinction Rousseau draws between physical or natural inequality and moral inequality is therefore founded on his understanding of the way our passions and faculties change character as we develop from physical to moral beings: "The same study of original man, of his true needs, and of the principles underlying his duties, is also the only good means one could use to remove those crowd of difficulties which present themselves concerning the origin of moral inequality" (1964, 96). The apparent historical structure of the *Second Discourse* with its progress from the pure state of nature to the state of nature to the civil state parallels and obscures a more fundamental argument about human nature and the (historical) conditions that shape it. The dichotomy between the physical and the moral runs throughout Rousseau's writings and is, I believe, the most fundamental argument about human nature in the *Second Discourse*.

While today we may not be so sensitive to the radical character of Rousseau's argument that we are by our original nature merely physical beings, the novelty and importance of Rousseau's argument about human nature was not lost on his contemporaries, as is made especially clear by one of the more orthodox readers of the *Second Discourse*, the Abbé Castel. Castel, an acquaintance of Rousseau's, was best known for his translations of Newton, his own work on optics, and his "ocular clavicord," which Rousseau ridicules in his *Essay on the Origin of Languages*. A year after the appearance of Rousseau's *Second Discourse*, Castel published a reply to the work, *L'homme moral opposé à l'homme physique de M. Rousseau*.[3]

As the title of Castel's work indicates, he takes particular exception to Rousseau's distinction between our physical and moral being and virtual glorification of the physical. He accuses Rousseau of seeing only the animal in man and not the image of God. He asks Rousseau at the outset, "Do you thus regard man as a wholly physical being?" and later calls him "the apologist of bestialness" (1782, 129, 182). "Nothing proves better that you harm religion," he admonishes Rousseau, "than that you take as positive the state of your solitary and animal savage as being the state of primitive innocence, as being the state itself of felicity itself and a terrestrial paradise" (pp. 144–45, 263–65). He contrasts Rousseau's solitary being in the pure state of nature with the social and familial being of Scripture and the natural law theorists ("It is not good for man to be alone," Genesis 2:18). He especially objects to the "brutality" of Rousseau's discussion of human sexuality and claims that the distinction Rousseau draws between physical and moral love clearly shows how he has misunderstood human nature (pp. 136, 159–89, 316–24). Finally, Castel perceptively observes that Rousseau's portrayal of man as a physical being that is somehow good is connected to his statement in the *Second Discourse* about "the little care taken by nature to bring men together." Far from "justifying" nature, as Rousseau claims to do through his work, Castel claims that his misdirected thesis about our physical nature as solitary and somehow good beings "calumniates nature itself, and God too" (p. 167).

From Rousseau's perspective, all of his predecessors have overestimated the needs, passions, and rational capacity natural to us. More importantly, they have assumed that we are naturally social or moral beings in one way or another—that we have social and moral passions and faculties. In other words, his predecessors have all assumed humans to be moral beings in the qualified sense he employs. Rousseau offers a radical reinterpretation of human nature through his portrait of natural man as a physical phenomenon in the pure state of nature. He presents a new understanding of our place in the natural whole that will, he claims, prove that we are naturally good, that our ills come from ourselves, and that nature is thereby justified.

NATURAL MAN AS PHYSICAL PHENOMENON IN THE PURE STATE OF NATURE

The Character of the Natural Whole

Rousseau begins his account of the pure state of nature with a description of natural man. Stripping him of "all the supernatural gifts he could have received and of all the artificial faculties he could only have acquired by long progress—considering him, in a word, as he must have come from the hands of nature," he finds "an animal": "I see him satisfying his hunger under an oak, quenching his thirst at the first stream, finding his bed at the foot of the same tree that furnished his meal; and therewith his needs are satisfied" (1964, 105). From the very outset of his portrayal of our natural condition, Rousseau shows us to exist peacefully and unproblematically in the abode of nature.

Rousseau's first portrait of our natural condition reveals a hospitable nature in which our needs are readily satisfied: "The earth, abandoned to its natural fertility and covered by immense forests never mutilated by the axe, offers at every step storehouses and shelters to animals of all species" (1964, 105). Nature and human intervention or art are here opposed: nature provides sufficient food and shelter for man and the other animals, while the axe "mutilates." Rousseau adds a note to his description of nature's plenty suggesting that it is Locke, in particular, whom he is opposing: "the fruits of trees furnish animals with more abundant food than other forms of vegetation can: an experiment I made myself, in comparing the products of two fields equal in size and quality, the one covered with chestnut trees and the other sown with wheat" (p. 187). This experiment appears to be derived from a comparison suggested by Locke to prove precisely the opposite point about natural fertility versus human cultivation. In England, Locke says, an acre of cultivated land "that bears here Twenty Bushels of Wheat" so that "the Benefit Mankind receives . . . in a Year, is worth £5" can be compared to the produce of the same acreage of comparable uncultivated land in America, the produce of which he says would be "possibly not worth a Penny; at least, I may truly say, not 1/1000 the value of the cultivated land" (*Second Treatise* 5. par. 43). Whereas Rousseau suggests nature's plenty and the destructive character of man's artful intervention, Locke implies a parsimonious nature that must be cultivated by us if it is be of any use. The full significance of Locke's argument about our place in nature and relation to nature's God—and thus of Rousseau's response to it—can be gleaned from a closer examination of Locke's theory of property as presented in the *Second Treatise*.

Locke initially speaks of the "Plenty God had given" us and his example of that plenty is the "Acorns he pickt up under an Oak." Acorns are Locke's first and archetypical example of natural appropriation of property by labor, in this case the eating of what is originally common, thereby making it intimately private.[4] Locke's state of nature seems at first to be one of provident plenty, but this impression is soon undermined. Acorns become Locke's primary example not only of man's natural right to appropriate the goods of nature through labor but of his absolute need to do so: they are representative of the scanty provision of nature. The initial appropriation of acorns as food is only the first sort of necessary labor. We must earn our bread by the sweat of our brows not because of any divine punishment but because of the character of the divine or natural whole. Locke uses the example of America given in his comparison of cultivated and uncultivated land: although nature has furnished the Americans "as liberally as any other people, with the materials of Plenty," because they have not cultivated the land, "a King of a large and fruitful Territory there feeds, lodges, and is clad worse than a day Labourer in *England*." He also recurs to the example of acorns as the "Plenty" given to these Americans by nature: "Acorns, Water, and Leaves, or Skins, must be our Bread, Drink and Clothing, did not *labour* furnish us with these [latter,] more useful Commodities. For whatever Bread is more worth than Acorns, . . . that is wholly *owing to labour*"; for "*labour makes the far greatest part of the value* of things, we enjoy in this World," since "Nature and the Earth furnished only the almost worthless Materials, as in themselves" (*Second Treatise* 5; see Pangle 1988). Locke's portrait of our unaltered place in nature and his emphasis on the benefits of appropriation or human art increasingly becomes an indictment of the providence of nature and nature's God. Rousseau's description of natural man "satisfying his hunger under an oak," in turn, is a vindication of the providence of nature.[5] In going back to the pure state of nature, Rousseau argues that Locke has misunderstood the character of the natural whole within which we exist.

More important than his argument about the character of the natural whole, however (for Locke allows that nature's provisions may be sufficient for man's bare needs), Rousseau objects that Locke and others have underestimated our ability to provide for ourselves in our natural condition and (most significantly) overestimated and mistaken the natural needs and passions for which we must provide: "Let us therefore take care not to confuse savage man with the men we have before our eyes" (1964, 111). Rousseau even imagines a fight between these two and argues that it would be tremendously unequal. This comparison is not only meant to support his argument that savage man can survive in his solitary existence in the pure state of nature but to point to a more fundamental difference between these beings. He concludes the comparison by explaining that savage man is an animal who (especially as compared to civil man) has "the advantage of always carrying oneself, so to speak, entirely with one" (p. 107). Savage man is an entirety, a being who lives as a physical whole in the natural whole. Rousseau explains the difference between civil and savage man similarly in *Emile*. Civil man, he says, "is only a fractional unity" whose "value is determined by his relation to the whole, which is the social body," while savage man "is entirely for himself. He is a numerical unity, the absolute whole" (1979a, 39–40).

Natural man is a purely physical being, and his relations with his fellows are limited to the physical. He discerns only "physical good and evil" and makes comparisons of "force" in his encounters with other beings (Rousseau 1964, 107). His fellow humans are equivalent in his mind to physical objects. Only when he develops moral passions, notably pride (*amour-propre*), do such encounters become more than purely "natural events" (p. 222). Until then he makes no moral judgments based on either intentionality or acquired notions of "the useful, the suitable, and the good" (see Rousseau 1979a, 39, 166–67, 213). Throughout his account, then, Rousseau argues that humans are limited by nature to needs for which they can easily provide and lack the needs and passions for which they might need others—moral needs and passions. His argument about the character of human needs, passions, and reason is the crux of his argument about human nature and his quarrel with his predecessors.

Human Needs, Passions, and Reason

Rousseau presents his account of the character and extent of our natural faculties and passions in explicit disagreement with Hobbes. He makes his criticism by utilizing the same basic understanding of the dynamics of human psychology that Hobbes himself employed against his predecessors—most importantly, the ancients and the natural law theorists. His argument with Hobbes about the extent and character of our natural faculties and passions points to their different understandings of our nature and place in nature.

In Hobbes's account of the state of nature, man's asocial and self-regarding passions make that condition a state of war not only in the juridical sense of lacking a superior to judge or of being "without a common Power to keep them all in awe" but also in the more tangible sense of living in an uncertain and violent condition of "continuall feare, and danger of violent death; And the life of man, solitary, poore, nasty, brutish, and short" (*Leviathan* 13). Humans are asocial in the sense that each individual looks primarily to his own self-preservation. This asociality results in a contest over scarce natural resources for both present and future consumption or enjoyment. Foresight and the related quest for power to obtain goods makes all resources, in principle, scarce. Hobbes concludes, "I put for a generall inclination of all mankind, a perpetuall and restlesse desire of Power after power, that ceaseth only in death" (chap. 11). Men are also self-regarding: they have a "mutual will of hurting" arising from both fear and vainglory. There are thus three main sources of quarrels: competition, diffidence, and glory (scarcity, fear, and pride). The very passions and faculties that make the state of nature incommodious Hobbes points to as separating humans from the beasts: he describes man variously as the sole animal with *vanity*, especially in contending for preeminence in reason; *speech* in the sense of a conventional art of words; and both *reason* and the natural *passion of curiosity*—foresight about causes and effects in general and thus the ability to develop Science (chaps. 2–6, 12, 27). The faculties Hobbes points to as distinctive of humans obviously require social relations. When he claims that humans are naturally asocial, then, Hobbes does not deny that they live in constant, if unstable, relations with their fellows. He means that individuals are naturally selfish and self-regarding, or prideful (see *De cive* 1.2, addendum). Indeed, Hobbes's description of the state of nature seems to presuppose that we normally or naturally live in some sort of families and social groups (see *Leviathan* 20). By pushing Hobbes's argument about our natural asociality even further, Rousseau comes to deny that we possess the speech or reason that Hobbes believes is not only natural to humans but distinctive of them.

Rousseau first introduces his quarrel with Hobbes when he imagines the "unequal fight" between a savage and a civil man: "Hobbes claims that man is naturally intrepid and seeks only to attack and fight"—against which, Rousseau cites Cumberland and an "illustrious philosopher" (Montesquieu) that "nothing is so timid as man in the state of nature" (1964, 106–7).[6] He suggests in a note to this passage that Hobbes has mistaken savage man for civil man because he has incorrectly understood the natural state of the human psyche: "All knowledge that requires reflection, all knowledge acquired only by the linking of ideas and perfected only successively, seems to be altogether beyond the reach of savage man for want of communication with his fellow men—that is to say, for want of the instrument which is used for that communication and for want of the needs which make it necessary" (pp. 188–89). As a being who carries himself entirely with himself, savage man does not require the assistance of his fellow humans physically, emotionally, or otherwise. His thorough integrity is based most importantly on his lack of the "needs" that lead to association with his fellow humans in the first place.

Rousseau develops his argument against Hobbes about the natural state of our minds by adopting the general theory of the dynamics of the mind employed by Hobbes, Locke, and others against their own predecessors. "Whatever the moralists may say about it," Rousseau begins, "human understanding owes much to the passions, which by common agreement also owe much to it" (1964, 115–16). The moralists assume in one way or another that humans are naturally and in some way primarily rational. They argue that we are universally subject to the moral dictates of the natural law as discovered by reason. Following Hobbes, in particular, then, Rousseau criticizes these thinkers for establishing the natural law so that "it is impossible to understand the law of nature and consequently to obey it without being a great reasoner and a profound metaphysician: which means precisely that men must have used, for the establishment of society, enlightenment which only develops with great difficulty and in very few people in the midst of society itself" (p. 94). As we have seen, however, Rousseau's own definition of a moral being is more capacious. He therefore pushes the psychological theory of Hobbes and Locke further, accusing them of failing to realize that humans are not by nature moral beings in any sense.

Rousseau plumbs further the insight of Hobbes and Locke by analyzing the relationships among needs, passion, and reason. First of all, the passions influence reason: "It is by their activity that our reason is perfected; we seek to know only because we desire to have pleasure." These passions, in turn, are based on desires; hence, "it is impossible to conceive why one who had neither desires nor fears would go to the trouble of reasoning." These desires are based on "needs," both natural ("by the simple impulsion of nature") and conventional ("through the ideas one can have of them"). Man is by nature limited to his natural needs: "His desires do not exceed his physical needs: the only goods he knows in the universe are nourishment, a female, and repose; the only evils he fears are pain and hunger. I say pain and not death and its terrors because an animal will never know what it is to die; and knowledge of death is one of the first acquisitions that man has made in moving away from the animal condition" (1964, 115–16). Rousseau's remark about man's lack of any fear of death is directed against Hobbes, who would make the fear of death the most forceful element of human psychology. To his statement that natural man's desires "do not exceed his physical needs," Rousseau attaches a short note that elucidates his disagreement with Hobbes and others:

> That seems totally evident to me, and I am unable to conceive whence our philosophers can derive all the passions they impute to natural man. With the sole exception of the physically necessary, which nature itself demands, all our other needs are such only by habit, having previously not been needs, or by our desires; and one does not desire that which he is not capable of knowing. From which it follows that savage man, desiring only the things he knows and knowing only those things the possession of which is in his power or easily acquired, nothing should be so tranquil as his soul and nothing so limited as his mind. (p. 213)

Rousseau turns Hobbes's reasoning against the moralists against him. "Hobbes saw very clearly the defect of all modern definitions of natural right," Rousseau says, but he "improperly included in the savage man's care of self-preservation the need to satisfy a multitude of passions which are the product of society and which have made laws necessary. . . . Hobbes did not see that the same cause that prevents savages from using their reason, as our jurists claim, prevents them at the same time from abusing their faculties, as he himself claims" (1964, 129). Hobbes and (less so) Locke describe our natural condition as improvident not only because of the scarce provision of natural goods but also because of the rich provision of contentious passions. Rousseau argues, in turn, that our unique "faculty of self-perfection, a faculty which, with the aid of circumstances, successively develops all the others" (p. 114), is activated only through the social or moral relations that arise from the contact with our fellows that we lack in our natural condition; that is, our uniquely malleable needs and passions develop and change their character to become moral needs and passions. As basically solitary beings, humans are devoid of reason, speech, foresight, contentious passions, and pride. The absence of these faculties and passions is what most importantly enables us to live a basically solitary and peaceful existence in our natural condition. By denying that we naturally have these needs, passions, and faculties, Rousseau is again able to describe the pure state of nature as a providential condition.

Sex and Moral Proofs in Matters of Physics

Rousseau cannot deny that humans completely lack needs or passions for one another, for the species depends upon them. Rousseau remarks that among man's "physical needs" is "a female." In the *Second Discourse* Rousseau speaks, at least initially, almost always of "man"—the male.[7] He speaks almost exclusively of males in his description of savage man's existence in the pure state of nature because he wants his account to be as far as possible genderless (as he sees it). "In everything not connected with sex," he claims in *Emile*, woman is man" (1979a, 357). In the *Second Discourse*, he explains that only after the establishment of families was "the first difference was established in the way of life of the two sexes, which until this time had had but one" (1964, 147). Rousseau describes natural man as a male at least in part because males can more easily be conceived of as solitary. When females are considered as females, questions arise concerning the ability of mothers to survive alone while caring for infants, the relatively long period of infancy among humans, and thus the possible need for relatively stable families and perhaps more extended social groups if humans are to survive.

Rousseau's psychological argument is presented in tandem with his characterization of our basically solitary natural existence and may even appear to depend upon it. However, he does not absolutely insist on his claim about our natural solitude. For example, he summarizes his argument about man in his original condition, "Let us conclude that wandering in the forests, without industry, without speech, without domicile, without war and without liaisons, with no need of his fellowmen, likewise with no desire to harm them, *perhaps* never even recognizing anyone individually, savage man, subject to few passions and self-sufficient, had only the sentiments and intellect suited to his state" (1964, 137; emphasis mine; see also p. 119). This passage also reveals that it is less important for Rousseau that his savage be solitary than that whatever minimal relations he might have with his fellow humans do not serve to develop his mental and psychological state. Rousseau's argument is thus not primarily that savage man does lead a solitary existence but that he is capable of doing so because of the character of his faculties and passions (see Masters 1968, 130–36, 172–75).

Rousseau directs his argument that constant relations between men, women, and children are not natural against Locke, who seems to assume the existence of relatively stable families and other social groups to be characteristic of our natural condition. How Rousseau makes his argument further elucidates his conception of the difference between our physical and moral being and of the mistake of Hobbes, Locke, and others.

Rousseau's discussion of the character and extent of sexual and familial relations in the pure state of nature is an outgrowth of his description of the relations among needs, passions, and reason. Reason and speech are intimately related. Languages obviously require some sort of social relations, and the family is the place we most associate with learning to speak. Rousseau thus raises the problem of language in conjunction with his denial of any stable social or familial relations in the state of nature. He offers his own description of life in the pure state of nature: "In the primitive state, having neither houses, nor huts, nor property of any kind, everyone took up his lodging by chance and often for only one night. Males and females united fortuitously, depending on encounter, occasion, and desire, without speech being a very necessary interpreter of the things they had to say to each other; they left each other with the same ease" (1964, 120–21). In a note to this statement, Rousseau transcribes at length a passage from Locke's chapter "Of Political or Civil Society" in the *Second Treatise*.[8] The part of passage to which Rousseau replies in detail pertains to Locke's claim about the nature of our sexual and familial relations, apparently even in the state of nature. Locke argues that the "chief, if not the only, reason" for the longer conjunction of male and female among humans than other animals is "because the female is capable of conceiving, and is commonly with child again and brings forth, too, a new birth, long before the former is out of dependency for support on his parent's help, and able to shift for himself," so that the father, in order "to take care for those he hath begot, and to undertake that care for a long time, is under an obligation to continue in conjugal society with the same woman by whom he has got them, and to remain in that society much longer than other creatures" (Rousseau 1964, 213–14).

Most importantly for the present purpose, Rousseau undermines the assumptions that he claims Locke makes about our natural psychological state: "Locke proves at most that there could well be in a man a motive for remaining attached to a woman when she has a child; but he does not prove at all that he must have been attached to her before the delivery and during the nine months of pregnancy." According to Rousseau, therefore, Locke "evidently supposes what is in question"—not only the existence of some kind of relatively stable society, which would provide the basis for such knowledge and foresight, but the very reason and foresight in the first place. Rousseau states that "this kind of memory, by which one individual gives preference to another for the act of procreation, requires, as I prove in the text, more progress or corruption in human understanding than can be supposed in man in the state of animality in question here" (1964, 218–19).

When he alludes to what he "proves" in the text, Rousseau is referring to the distinction he draws between physical love and moral love somewhat after this note. There he asserts that by nature the sexes have no need for any prolonged association because they have no more than an occasional need to couple in order to preserve the species. Physical love, or simple copulation, is limited to purely physical impulses and relations. Moral love, in turn, is based on reasons and opinions conceived through the imagination; and these only develop in society. Limited to the physical in love, male and female in their natural state meet sporadically and, with their physical needs satisfied and no knowledge of the possible consequences of their act, they return to their basically solitary existence (1964, 134–36).

The distinction Rousseau draws between physical and moral love is part of the more general dichotomy between the physical and the moral in the *Second Discourse*. His allusion to what he "proves" about physical and moral love helps us to understand what he says at the outset of his criticism of Locke in the note: "I shall observe first that moral proofs do not have great force in matters of physics, and that they serve rather to give a reason for existing facts than to prove the real existence of those facts" (1964, 215–16). Once again, Rousseau is accusing Locke of making the same sort of mistake as the natural law theorists, or those whom Rousseau earlier termed the moralists. Assuming humans always to have been the beings he sees around him, Locke has improperly attributed to us faculties and passions that (Rousseau argues) we do not possess by our original nature.

Rousseau reiterates his general criticism of Locke at the very end of the note in a different form:

> Locke's reasoning therefore falls apart, and all the dialectic of the philosopher has not saved him from the error committed by Hobbes and others. They had to explain a fact of the state of nature, that is to say, of a state where men lived isolated and where a given man has no motive for living near another given man, nor perhaps to live near one another, which is much worse; and they did not think of carrying themselves back beyond the centuries of society, that is to say, of those times when men have always had a reason to live near one another, and when a given man often has a reason for living beside a man or a given woman. (1964, 219–20)

Despite his suggestion that Locke and Hobbes have made a historical mistake, not "carrying themselves back beyond the centuries of society," Rousseau's argument against Locke's understanding of man's *psychological* state is evident, as well. When he criticizes Locke for not going "back" far enough, he indicates that Locke has not properly conceived of how different conditions (conditions that tend to develop in a certain general direction over time) affect the character and extent of man's "needs." The change that takes place in man's "need" for his fellow human, especially that for a particular member of the opposite sex, is crucial. (Note the change from two given men in the first state to a "given man" and a "given woman" in the second.) In other words, Rousseau denies that humans by nature have what he calls moral needs or passions—needs and passions with particular, conscious regard for other individuals, of which moral love is his archetypical example.

Rousseau's argument about moral proofs in matters of physics can be generalized and applied to his criticism of the views of Hobbes and Locke (among others) about mankind's relation to the natural whole within which they exist. The state of nature as envisioned by Hobbes and Locke hides within it a cryptoteleology. Supposing that because of the character of nature or because of the passions natural to humans the state of nature is inimical to us, their state of nature is paradoxically teleological in that its insufficiencies point to another condition that is in a sense higher—the end of the state of nature through the artifice of the social contract. Rousseau, in contrast, describes how we can survive as purely physical beings in our natural state, as good beings that exist unproblematically in nature and that need never have developed out of that condition.

THE NATURAL GOODNESS OF MAN

Rousseau claims that he has demonstrated that man is naturally good and that he has thereby justified nature (1964, 193). He later explains that the natural goodness of man is central to his system of thought, as when he exposed the "fundamental principle" of all of his writings, namely, "that man is naturally good, loving justice and order, that there is no original perversity in the human heart, and that the first movements of nature are always right" (1959–69, vol. 4, pp. 935–36).[9] Rousseau's demonstration of the natural goodness of man in the *Second Discourse* consists in his portrait of savage man's natural condition in the pure state of nature as a physical being embedded in (physical) nature. Before turning to the meaning of his claim to have thus justified nature and even God, let me take up what Rousseau means by our natural goodness.

We usually brand attributions of goodness as moral judgments—even as matters of "value" distinct from those of "fact." Rousseau, however, does not use *good* in a moral sense and even takes it in a sense compatible with matters of physics. Rousseau's conception of goodness becomes clear when, after his description of the pure state of nature, he wonders what kind of mortality we could be said to possess by nature:

> It seems at first that men in that state, not having amongst themselves any moral relationship or known duties, could be neither good nor evil, and had neither vices nor virtues: unless, taking these words in a physical sense, one calls vices in the individual the qualities that can harm his own preservation, and virtues the those that can contribute to it; in which case, it would be necessary to call the most virtuous the one who least resists the simple impulses of nature. (1964, 128)

Although Rousseau immediately claims that he will speak "without departing from the ordinary meaning" of these words, it is clear, since he understands natural man to be a purely physical being, that he also means us to take these terms in the "physical sense" he suggests.

An indication of Rousseau's novel use of these terms is seen by the objections of the Abbé Castel. He complains that Rousseau's savage has "nothing of the moral, of human, of theological or divine; nor any more of duties, morals, relations; nor any more of good, that is to say, of bad. For here is the system of M. Rousseau well explained in detail and well announced: according to him, the good is bad and the bad is good" (1782, 182–83). Similarly, Diderot, appears to have been thinking along the same lines in his article "Droit naturel" for the *Encyclopedia*, which has been read as a reaction to Rousseau's arguments in the *Second Discourse* "If man is not free or if his instantaneous determinations, or even his oscillations, arise from something material that is external to his soul, he will have in them neither reasoned goodness nor wickedness, even though they may have in them animal goodness or wickedness" (1975, vol. 7, p. 25; see Masters 1968, 261–65).

We are good, says Rousseau, in that we do not resist "the simple impulses of nature." Through our spontaneous instinct for self-preservation alone and perhaps also restrained by natural pity, we are by nature good for ourselves (well-ordered and self-sufficient) and good, or at least not harmful, for others (see Melzer 1990, 15–17; see also Masters 1968, 151–57). We are good by the "two principles anterior to reason"—the desire for self-preservation and pity—which Rousseau explains he has found "by meditating on the first and simplest operations of the human soul" (1964, 95). Together they compose the "maxim of natural goodness": "Do what is good for you with the least possible harm to others" (p. 133).[10] We do not by nature need to force ourselves, through reason or otherwise, to act in a manner that is good. Such force, whether external force applied through education or threat or internal "self-forcing," Rousseau is fond of pointing out, is found in the original sense of *virtue* (see Rousseau 1979a, 444–45). He therefore reserves the terms *virtue* and *vice* for moral beings, that is, humans with the needs and passions that develop in and through their relations with their fellow humans and that cause them to be good neither for themselves nor others.

Rousseau makes his argument about the natural goodness of man in disagreement with Hobbes: "Above all, let us not conclude with Hobbes that because man has no idea of goodness he is naturally evil; that he is vicious because he does not know virtue; . . . nor that, by virtue of the right he reasonably claims to things he needs, he foolishly imagines himself to be the sole proprietor of the whole universe." Rousseau thus concludes, "One could say that savages are not evil precisely because they do not know what it is to be good: for it is neither the growth of enlightenment nor the restraint of law, but the calm of passions and the ignorance of vice which prevents them from doing evil" (1964, 128–30). Ironically, then, the Abbé Castel claims that even Hobbes is "not impious, in that he supposes man capable of impiety. Man having himself neither virtues nor vices, nor moral relations, nor known duties, could not be impious, whatever he should do, not being more than a brute beast and animal" (1782, 171–72). As a number of modern interpreters of Rousseau have noted, his savage man's goodness is amoral, premoral, or even subhuman (e.g., Masters 1968, 152; Starobinski 1964, lxiv; Strauss 1953, 271).

THE THEODICY OF THE *SECOND DISCOURSE*

Rousseau claims that through his portrait of our original nature as asocial, physical, and good beings that he has "justified" nature and even God and his providence. He raises the issue of providence in the *Second Discourse* just before alerting us to his use of *good* in a physical sense. He explains that "from the little care taken by nature to bring men together from mutual needs and to facilitate their use of speech, one at least sees how little is prepared their sociability, and how little it contributed to everything men have done to establish social bonds." Thus Rousseau, the Abbé Castel claims, calumniates God and nature; but Rousseau maintains that nature has thus been kind to us: we are placed in this natural condition "by a very wise providence." To the argument that such a being would be miserable, he says, "Now I would really like someone to explain to me what type of misery there can be for a free being whose heart is at peace and whose body is healthy?" (1964, 126–28). Rousseau reiterates the soundness of man's physical endowments, both mental and bodily, and the harmony in which he lives as an unproblematic whole in the natural whole.

Rousseau treats the question of divine or natural providence in his 1756 letter to Voltaire in response to the latter's poem on the Lisbon earthquake. How can human misery and death be consistent with a beneficent God or well-ordered nature? Rousseau points to the argument in the *Second Discourse*, a work Voltaire had characterized as a "work against the human race." No one, Voltaire wrote, had ever used so much intelligence to make man a beast (quoted in Rousseau 1959–69, vol. 3, 1379). Rousseau's answer is simple: "If it is better for us to be than not to be, this would be enough to justify our existence, even though we would have no compensation to expect for the ills that we have to suffer." Voltaire and others, he says, are wrong to accuse God or nature because "in the comparison of goods and evils," they "always forget the sweet sentiment of existence" (ibid., vol. 4, pp. 1062–63; see Rousseau 1979b, 68–69). In the *Second Discourse*, Rousseau describes the psychological condition of natural man in the pure state of nature: "His soul agitated by nothing, is given over to the sole sentiment of its present existence" (1964, 117). The providence of nature and nature's God exists for us in our natural condition as physical beings unproblematically embedded in the physical whole of nature.

In his letter to Voltaire, Rousseau explains that God's providence is impersonal, "general," and wholly physical in character, that is, not specifically concerned with the individual: "It is to be believed that particular events are nothing here below in the eyes of the master of the universe, that his providence is only universal, that he is content to preserve the genera and the species and to preside over the whole without being disturbed by the manner in which each individual spends this brief life" (1959–69, vol. 4, p. 1069). Thus, even though Rousseau seems to assume a high infant mortality in our natural condition—"Nature . . . renders strong and robust those who are well constituted and makes all the others perish" (1964, 106; see p. 111)—what is more important is the survival of the species: "The proof that all is well ordered derives from a general and incontestable fact: that all the species survive" (1959–69, vol. 3, p. 237). What is most important in considering the providence of nature or nature's God is the order of the whole. In almost equating goodness with order, both in his discussion of the natural goodness of man and the goodness of nature as a whole, Rousseau appeals to a traditional theological concept. In the "Profession of Faith of the Savoyard Vicar" in *Emile,* for example, he has the vicar explain that "the love of order which produces order is called *goodness;* and the love of order which preserves order is called *justice*" (1979a, 282; emphasis original). He alludes to this same conception in the *Second Discourse* when he writes that men do not naturally have the reason to conceive of "maxims of justice and reason drawn from the love of order in general or from the known will of his creator" (1964, 119). The order of nature, including human nature, is found in our original or natural condition as physical beings embedded unproblematically in the physical whole of nature. We are thus a good part of a good nature: "The first movements of nature are always good and right" (1959–69, vol. 4, pp. 935–36; see Rousseau 1979a, 92).

Previous interpreters who have attended to Rousseau's understanding of his work as a theodicy have come to a different conclusion. Rather than understanding Rousseau's conception of our natural goodness and his justification of nature or God as referring to our original position in nature, they have seen his solution to the problem of evil and providence as eschatological: as coming at the end as a restitution of order. This view gains credence from the argument about providence made by Rousseau's Savoyard vicar. Chief among these interpreters is Ernst Cassirer. Cassirer correctly sees that Rousseau's unique solution to the problem of evil (society as the cause of our ills) is central to his thought. However, he incorrectly believes that Rousseau's resolution of the problem of evil is found in our ultimate reformation in the ethical society where the categorical imperative (the general will) reigns. For Rousseau, he asserts, the goodness of man "was not an original quality of feeling but a fundamental orientation and a fundamental destiny of man's will" and was grounded "in man's capacity for self-determination . . . in the recognition of an ethical law" (Cassirer 1989, 104–5; see also Cassirer 1951, 147, 153–58 and Hendel 1934). More recently, Andrzej Rapaczynski (1987) has made a similar argument, finding in the vicar's eschatological providentialism a clue to Rousseau's thoughts about the human problem.

Cassirer and others have erred, first, because they have misunderstood or misrepresented Rousseau's conception of our historical nature. Seeing in Rousseau the predecessor to Kant, Hegel, and even Marx, all of whom in different ways viewed providence as a historical phenomenon (see Kant 1983, 53–55), Cassirer and others' interpretations of Rousseau have a distorted, proleptic character. Rousseau does not view history primarily as the process of our perfection (for he is decidedly pessimistic about our "progress") but the addition of encrustations on our original nature through changes in the determinants of the extent and character of our malleable passions and faculties, our historical being. He reveals our original condition and our historical nature so that we may see that our ills are not inevitable, that we are good, and that nature is justified. His portrait of nature and of human nature is useful not as a focus of nostalgia but as a model for the conditions of our happiness, in accordance with which it is possible to manipulate the determinants of our nature.

Second, they have erred because their interpretations are based, in part, upon a mistaken assumption about Rousseau's intentions in offering the teaching about providence he has the Savoyard vicar offer and the relationship of that teaching to his own understanding. The vicar argues that an eschatological and restitutive moral providence is the solution to the problem of disorder and evil among men (1979a, 278–82). Cassirer and others have seized upon the vicar's argument as substantially Rousseau's own. The exact relationship between Rousseau and his vicar is an extremely difficult and much-debated subject. However, Rousseau's remark in his letter to Voltaire that our existence is justified "even though we would have no compensation to expect for the ills that we have to suffer" (i.e., even without a doctrine of compensation from a caring deity) indicates that he offers a strictly natural justification of nature and providence.[11] Finally, Rousseau's disagreement with the vicar about the "end" at which providence is found or justified is attested to by the sole reference to the "Profession of Faith" elsewhere in *Emile.* In a passage that explicitly recalls the *Second Discourse* (where he also states that Hobbes called the wicked man "a robust child," p. 129), Rousseau explains, against Hobbes, that children are naturally good: "All wickedness comes from weakness. . . . Of all the attributes of the all-powerful divinity, goodness is the one without which we can least conceive it . . . . See hereafter the Profession of Faith of the Savoyard Vicar" (1979a, 67). The vicar states that "the supremely good Being, because He is supremely powerful, ought also to be supremely just" and goes on to relate goodness with order and justice with the preservation of order. The context of the reference earlier in the work points to Rousseau's solution of the problem of evil and understanding of providence as lying in our beginning as a good part of a good nature.

The notion of providence seems to require an intelligent and willing being and thus to be misapplied to nature. Just as his quarrel with Hobbes shows how Rousseau could speak of goodness in a physical sense, so it reveals what sense it makes to speak of natural "providence." This can be seen by contrasting Rousseau's view of human nature and our place in nature with Hobbes's.

Hobbes discusses the issue of providence and man's place in nature and relation to God as the "author of nature" in *Leviathan* 31, "Of the Kingdome of God by Nature." Hobbes notes the metaphorical application of the term *providence* to the "Power of God" as the "Author of Nature." Nonetheless, he proceeds to consider "the Right of Nature, whereby God reigneth over men, and punisheth those that break his Lawes," which he reduces to God's "Irresistible Power." Central here is Hobbes's reduction of right to power, evident earlier in the work in his discussion of natural right (chaps. 14, 21; see also Spinoza, *Theologico-Political Treatise* 6, 16). Rewards and punishments are central to every conception of providence, most especially the rewards and punishments of an afterlife thought necessary to resolve the problem of evil and justify God and his providence: "Punishments are consequent to the breach of Lawes; Natural Punishments must be necessarily consequent to the breach of the lawes of Nature; and therefore follow them as their naturall, not arbitrary effects" (*Leviathan* 31). However, with Hobbes's essential reduction of natural law to a "general rule of reason" or prudence, and incorporation of natural law into natural right (chap. 14), natural rewards and punishments become equivalent to simple pleasure and pain as events in the "chayn of Consequences" of our actions or as consequences of our motion simply (chap. 31). For Hobbes, however, our natural condition is a state of war of every man against every man because of the passions and faculties natural to us. Though intended to procure pleasure or at least avoid pain, our actions produce pain and even death. Our natural condition is not a providential one in any positive sense. Rather than ordered beings in an ordered whole, Hobbes finds beings whose natural impulses lead them to disorder and death.[12]

Like Hobbes, Rousseau's conception of natural providence is metaphorical. Unlike Hobbes, however, natural providence has a positive character. For Rousseau, our natural passions and needs lead to our survival and even flourishing. Only when we depart from nature with our fellows out of the hands of nature's providence does a process begin that makes "a being evil while making him sociable, and from such a distant origin finally bring man and the world to the point where we see them" (1964, 140). Or, as Rousseau explains of his system, all of his writings have been founded on "the great principle that nature made men happy and good, and that society depraves him and makes him miserable" (1990a, 213). Rousseau's portrait of our natural, providential condition in the pure state of nature serves as a positive model for the remaking of our miserable existence.

THE BEARING OF ROUSSEAU'S PURE STATE OF NATURE

The state of nature constitutes what might be called a negative standard in the political theories of Hobbes and Locke. The state of nature is no longer a hypothetical prelapsarian state of innocence created by God and lost by men as in the scriptural and natural law traditions that they confronted, but a state of want and war. The state of nature in their theories is far from the creation of a provident Deity; it is scanty in its physical provision and rich in its provision of quarrelsome human passions. Hobbes in particular replaces the reasoned duties and ends of the natural law theorists with the solid and sublime beginnings of the passions, especially the fear of pain and death. Reason serves as the guide—the star and compass—to the ends determined by the passions. Right—each individual's natural right to self-preservation and the means to it—is prior to duty. But thus, there appear to be no true ends in themselves: the most important end is paradoxically the beginning point of *the* end—death. Life becomes characterized by a desire to maintain motion; the most important goal is to avoid the end of motion.

The negative standard of the state of nature that may be said to be characteristic of the thought of Hobbes and Locke affects the bearing of liberal thought. The primacy of natural right along with the state of nature with which it is frequently allied might be said to provide no positive standard for politics.[13] They provide to the natural individual a sort of fence that cannot be crossed or a way of judging the relationship between the individual and the state. They do not tell us what sort of political entity to construct except (for the most part) to say what cannot be done. They do not offer any, or much, advice about what sort of life the citizens of the state should lead or what type of beings they should be. There is in liberal thought and its emphasis on natural right a tendency, as in Hobbes, to provide for the continuation of motion without giving that motion any determinate direction or end. The existence of the individual is characterized by the *pursuit* of happiness, while happiness itself remains indeterminate.

Rousseau shares much with his predecessors in the liberal tradition of the state of nature and the social contract. For example, his state of nature also eventually becomes a condition of want and war that must be overcome through art or convention. However, he goes further back to the pure state of nature, a new revelation of what sort of beings we truly are by nature. His natural man is a physical being who exists unproblematically as a good being embedded in a good nature. He is a being characterized by wholeness. Rousseau's pure state of nature and our place within it are positive standards, of sorts, for his constructive philosophy, which is concerned not only with life, liberty, and the pursuit of happiness but with happiness itself. He envisions the palliation or reconstruction of our corrupted existence by remaking our malleable or "perfectible" nature by imitating our natural condition as whole beings embedded unproblematically in the whole of nature—albeit on different foundations from those of our physical existence in the pure state of nature. Thus, although Rousseau calls the savage societies of the state of nature the "happiest and most durable epoch" for mankind (1964, 151) and although some interpreters have recognized how those primitive societies serve as a model for at least a portion of Rousseau's constructive thought (e.g., Shklar 1985), still, even that community with its relatively whole or unified beings living in a whole shares a formal similarity with our natural existence in the pure state of nature.

That Rousseau views the existence of natural man as a positive standard has, of course, often been noted. So far, however, interpreters have concentrated on the unity of soul of the individual alone as Rousseau's standard (Melzer 1990; see also Strauss 1953). They have failed to understand how his view of our place in nature in general serves as a positive standard, a theodicy. This criticism applies to Starobinski's (1988) somewhat similar interpretation; for his reading of Rousseau as concerned with a "transparent" relationship of an individual with his fellows and surroundings and the problem of the "obstruction" in that relationship is at bottom a radically individualistic reading of Rousseau (as should be evident from his psychoanalytic approach to Rousseau to begin with). Only the present interpretation can fully account for both the unity of intention and similarity of method behind the various solutions Rousseau offers in his constructive philosophy. He entertains three distinct and ultimately problematic possibilities of our "perfectibility," all in some sense imitations of the savage man's existence in the pure state of nature: the solitary walker (*Reveries of the Solitary Walker*), the retreat of the domestic society (*Emile* and *Julie, ou la nouvelle Héloïse*), and the legitimate political association (*Social Contract*). Let me say something about Rousseau's political solution for our ills.

THE PURE STATE OF NATURE AND ROUSSEAU'S POLITICAL PHILOSOPHY

Rousseau's political teaching in the *Social Contract* seems to be elaborated independently of the psychological and historical investigation of the *Second Discourse* and, indeed, of the main tenets of his system of the natural goodness of man. Thus, even where the historical and psychological arguments appear in his explicitly political work, they are phrased narrowly: the social contract becomes necessary when the obstacles to self-preservation become too great (e.g., *Social Contract* 1.6). However, as Arthur Melzer (1990) has perceptively argued in his recent work, the cast of Rousseau's argument is misleading. Most importantly, Rousseau can be seen to be concerned in the *Social Contract* to alleviate, among other things, the main cause of our evils as we have developed into moral beings, namely, personal dependence. "Man is born free, and everywhere he is in chains. One who believes himself the master of others is nonetheless a greater slave than they," Rousseau announces in his opening words. Even mastery is a form of slavery because of personal dependence. Although his paradoxical statement that "whoever refuses to obey the general will shall be constrained to do so by the entire [political] body; which means only that he will be forced to be free," is infamous, its continuation is less well known: "For this is the condition that . . . guarantees him against all personal dependence" (1.8). As Melzer notes, Rousseau can present his political teaching in the *Social Contract* as though it were separate from the main tenets of his system and based on the narrow problem of self-preservation, because "the political requirements of preservation and happiness perfectly coincide" (1990, 114–15). Building on Melzer's interpretation, I shall add that in his political thought, Rousseau intends to solve the problem of personal dependence by making the state an imitation of the divine or natural whole.

Rousseau would palliate the ills from personal dependence by making the citizens of his regime as far as possible independent of one another and dependent upon the laws alone. The laws ought to make the relationship of the citizens to one another "as small as possible" and the relationship of the citizens to the state and its laws "as large as possible" (*Social Contract* 2.12). The independent civil freedom of the citizens in Rousseau's state is an imitation of the independent natural freedom of the savage in the pure state of nature (see 1.8, 2.11). Rousseau intimates in *Emile* how the relationship of the citizens to the political whole within which they live is intended to be an imitation of savage man's existence in the pure state of nature:

> Dependence on things, since it has no morality, is in no way detrimental to freedom and engenders no vices. Dependence on men, since it is without order, engenders all the vices, and by it, master and slave are mutually corrupted. If there is any means of remedying this ill in society, it is to substitute law for man and to arm the general wills with a real strength superior to the action of every particular will. If the laws of nations could, like those of nature, have an inflexibility that no human force could ever conquer, dependence on men would then become dependence on things again; in the republic all the advantages of the natural state would be united with those of the civil state, and freedom which keeps man exempt from vices would be joined to the morality which raises him to virtue. (1979a, 85)

In the first draft of the *Social Contract* and in a parallel passage in his essay "Political Economy," Rousseau explains, in his discussion of "the marvels of the laws," that we must "imitate here on earth the immutable decrees of the divinity." "It is this celestial voice" of the laws, he explains, "that tells each citizen the precepts of public reason, and teaches him to behave according to the maxims of his own judgment and not to be constantly in contradiction with himself" (1978, 177–78).

The laws of Rousseau's political whole are to be modeled on the laws of the divine or natural whole; and our relationship to them is similar except that in the case of the civil state, the citizens themselves may legislate them through our general will as sovereign. The parallel between the political whole and the divine or natural whole in Rousseau's conception is further strengthened when we consider that the origin of the general will, the source of the laws, as a theological term concerning the divine will and providence, a heritage most thoroughly investigated by Patrick Riley (1986). The "voice of the people is the voice of God," Rousseau exclaims in his "Political Economy" (1978, 213; see Rousseau 1959–69, vol. 3, pp. 862–63), giving the traditional formula a new and radical meaning.

CONCLUSION

I began by wondering about the relationship between Rousseau's description of human nature through his depiction of our natural condition in the pure state of nature and his political thought. Despite certain appearances to the contrary, his juridical political theory and the legitimate state he derives from it, far from having been developed in isolation from his demonstration of the natural goodness of man, depends decisively upon it. Rousseau's projected solutions to our ills, including his political remedy, are formally modeled on the natural condition of the human race as a good part of a good nature—as we can see when we understand that Rousseau intends the *Second Discourse* to be a theodicy.

The connection between Rousseau's reflections on human nature and his political theory force us to reconsider how these subjects may be inseparable, even at the point in the history of political thought where they appear to diverge. Indeed, since Rousseau's understanding of human nature is bound up with his conception of nature and questions about the divine, he offers an opportunity to consider a return to the grand philosophical tradition in politics. As our liberal politics and political science triumph, it is imperative that we come to understand their foundations better, build on them more solidly, assess their possible structural limitations, and grasp their possibilities.

Notes

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1. Throughout this paper I will discount the quasi-Lamarckian theory of the changes in our physical constitution through time that Rousseau seems to contemplate in the *Second Discourse* but puts aside (1964, 104–5, 183–86, 203–9). In any case, if Rousseau puts forward his hypotheses about human history in general and changes in our constitution in particular in a scientific sense (as I believe he does), the thesis that I will maintain is the main one of the *Second Discourse*, namely, that we are by nature physical beings, would only be strengthened.

2. For background on the state of nature and the pure state of nature, see Gierke 1950, 96–105 and notes; Gourevitch 1988, 25–33; Strauss 1953, 184–85. For the original formulations, see Aquinas, *Summa theologica* I–II. 109.8, as well as 102. 3–12, 109. 2–4; Suarez, *De legibus ac Deo legislatore* 1.3.11–12, 2.8.8–9; Hooker, *Of the Laws of Ecclesiastical Polity* 1.10.1; Grotius, *De jure belli ac pacis* 2.5.15, 3.7.1; Pufendorf, *De officio hominis et civis* 2.1.1–7; idem *De jure naturæ et gentium* 2.2.1–4.

3. Castel's work was originally published in Toulouse in 1756 as *L'homme morale opposé à l'homme physique; ou Réfutation du discours sur l'origine de l'inégalité: Lettres philosophiques*. It is included in a supplemental volume (no. 29) to the 1782 *Collection complètes des oeuvres de J.-J. Rousseau* edited by Moultou and Peyrou, to which I refer here. My attention was first drawn to Castel by Hamilton 1979, 86–89.

4. Locke perhaps chooses acorns as his representative example of property by appropriation because of Pufendorf's parallel use of acorns in his theory of property deriving from the universal consensus of mankind (*De jure naturae et gentium* 4.4.13). This parallel was drawn to my attention by Peter Laslett (Locke 1988, 288n.), who, in turn, takes it from John Gough (1950, 78n.). The centrality of the oak and acorn to descriptions of man's primitive state is also to be found in Lucretius: "Their hearts were content to accept as a gift what the sun and showers had given, and the earth had produced spontaneously. Often they stayed their hunger among the acorn-laden trees" (*De rerum natura* 5.937–40). It may have been Lucretius who suggested this image to Pufendorf, who quotes this passage from Lucretius at length in his *De jure naturae et gentium* (2.2.2). It has often been noted that Rousseau's depiction of man's natural state is modeled on the fifth book of Lucretius' poem (e.g., Morel 1909; Strauss 1953, 264, 271); and among the many parallels between the accounts is this picture of man resting beneath an oak. However, although Lucretius and the other Epicureans, like Rousseau, deny natural teleology, their account of man is not wholly antiteleological: unlike Rousseau, they locate our natural condition not in our beginnings but in our fulfillment, or *summum bonum*, which is not found in our beginnings (see Melzer 1990, 122). Rousseau thus understands the Epicureans to "deny all providence" (1959–69, vol. 3, p. 46n.).

5. I shall use the metaphor of "providence" when analyzing nature in Rousseau's understanding, for this is what his claim in the *Second Discourse* to have "justified" nature must mean. The account in the *Second Discourse* is a strictly natural one, hence Rousseau does not explicitly discuss God or divine providence in the work. In his 1756 letter to Voltaire, which I shall discuss, he extends his claim about the *Second Discourse* and says that he has justified God and divine providence in some way, as well.

6. By citing Montesquieu against Hobbes, Rousseau might be said to exploit a breach in Hobbes's theory of the state of nature opened but not fully exploited by Montesquieu. In the first book of his *Spirit of the Laws*, Montesquieu takes exception to Hobbes's characterization of man's polemic nature: "But one feels that what can happen to men only after the establishment of societies, which induced them to find motives for attacking others and for defending themselves, is attributed to them before that establishment." By Hobbes's premises about the primacy of the desire for self-preservation, Montesquieu says, one should, instead, see that man is extremely diffident, not quarrelsome, in his natural state (1.2;

see 30.19). Rousseau's complaint against Hobbes and Locke is similar. Montesquieu does not, however, provide an explanation of how man developed from that condition to the one described by Hobbes and acknowledged by Montesquieu as later (though not originally) correct. Rousseau may be said to offer such an explanation in the *Second Discourse*. His explanation is both historical (How did diffident and basically solitary men come together in the savage societies that eventually decline into a condition of war?) and, more importantly, psychological (How did man change from a timid and wholly asocial being to a contentious and other-regarding being?).

7. This apparently highly gender-specific description of the savage has been noted by a number of readers and received different reactions. Some have chastised Rousseau for taking the male as somehow representative of humanity and excoriated him for taking the female to be the deficient sex, in particular, in his statements about the education of Sophie in *Emile* 5 (e.g., Keohane 1980). Others applaud him for what is seen to be an ultimately genderless description of savage humans and accuse him of inconsistency in his discussion of the differences and inequalities between the sexes after they have developed (e.g., Okin 1979, 108–11). Without attempting here to defend or accuse Rousseau in these matters, it should be noted that his analysis of the differences between the sexes relies on his understanding of the interaction between nature and convention, especially of how conventional differences between men and women may or may not be founded on natural distinctions brought out only in unnatural conditions. In other words, Rousseau's discussion of human sexuality, whatever its ultimate validity, centers around issues still in contention.

8. Rousseau transcribes an anonymous French translation of Locke's *Second Treatise* 7. pars. 79–80. The translation is fairly close to the original and will be followed here as translated by Masters in Rousseau 1964.

9. While Rousseau claims that together his works form a "system," he is not a "systematic" philosopher in the sense, say, of such seventeenth-century thinkers as Leibniz or Malebranche. He distinguishes between the "systematic spirit" and the "spirit of observation": the former is characterized by the tendency to construct systems from abstract and universal rules and hasty overgeneralizations; the latter, to which he adheres, entails careful empirical observation and cautious generalizations (1959–69, vol. 4, pp. 1090–93). Rousseau's discussion of theodicy and providence is therefore, quite different from that of, Leibniz, for example, as Rousseau himself implies (see vol. 3, pp. 232–34). Whereas Leibniz might be said to begin from questions of metaphysics and even logic, reasoning deductively from a priori attributes of the divinity (e.g., in his *Theodicy*), Rousseau characteristically begins with the human problem and our very real ills. For the character of Rousseau's system, see Lanson 1912, 7; Machado 1968, 33–36, 83–85; Melzer 1990, 9–12; Starobinski 1988, 40–41.

10. While pity serves as the basis for human morality, it does not itself appear to be a moral sentiment or passion in Rousseau's understanding. There is some question to what extent Rousseau believed that pity would be active among the savage men of the pure state of nature. For at least reasons of exposition, the activity of pity he suggests in the *Second Discourse* (1964, 131–33) seems significantly different from his account in the *Essai sur l'origine des langues* (1990b, chap. 9) and *Emile* (1978, 221–23), where he denies its initial activity, although I would argue that close examination of his argument in the *Second Discourse* would remove this apparent contradiction. Finally, and in a manner that may explain both why pity is not a moral sentiment and the ambiguities of his presentation, Rousseau suggests in *Emile* that all the passions, apparently including pity, can be derived from the natural sentiment of self-love (1979a, 212–13).

11. I show at length elsewhere through a close analysis of his letter to Voltaire, the "Profession of Faith," and other writings that Rousseau offers a strictly natural account of the whole and our place in it (Scott 1992). Rousseau puts forward his arguments about an immortal soul and a caring Deity with particular moral providence as at best doubtful and indicates that he does not ultimately believe them. He presents these arguments as useful and even essential for the virtue and happiness of most individuals and all political communities. This is the spirit in which he discusses civil religion in the *Social Contract* (4.8).

12. Hobbes has a parallel discussion in *De cive* 15. Pufendorf locates divine providence in our being led to sociability through our natural reason and natural law (*De officio hominis et civis* 1.3.10–11).

13. This cannot be said without qualification (for at least Locke), however; for the natural freedom he shows us to enjoy in the state of nature is the model for his description of civil freedom. This is true of Montesquieu, as well (*Spirit of the Laws* 11.3–5). Rousseau's understanding of the relationship between natural and civil freedom is similar (*Social Contract* 1.8).

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POSTMODERNISM AND THE PUBLIC SPHERE

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The idea of the public sphere, of an institutionalized arena of discursive interaction, is central to democratic theory and practice. The modern age has, however, witnessed the erosion of a public realm distinct from the state and the market. In response to this erosion, public realm theory, notably the work of Arendt and Habermas, attempts to theorize the minimal conditions necessary for a discursive realm free of structural coercion or manipulation. The resulting normative conception of the public sphere has come under sharp attack by postmodern theorists, including Foucault, Lyotard, and Baudrillard, who question the basic presuppositions of public realm theory. I examine their objections and show how the public realm theory of Arendt can be viewed as motivated by concerns similar to the postmoderns'. Against Habermas, I argue that Arendt's public realm theory is less concerned with the question of legitimation than with the theorization of an agonistic political subjectivity.

As a motto, "the recovery of the public realm" captures, more or less adequately, the primary goal of Hannah Arendt's political philosophy and the critical theory of Jürgen Habermas. Both Arendt and Habermas see the public sphere as a specifically political space distinct from the state and the economy, an institutionally bounded discursive arena that is home to citizen debate, deliberation, agreement, and action. Yet they also see this sphere as overwhelmed by the antipolitical forces unleashed by modernity. For Arendt, the modern rise of "the social" effaces what was once a strong distinction between the public and the private and tears down the boundary between the realms of necessity and freedom (1957, 33–49). The result is that the public sphere is "devoured" by "household" concerns—the maintenance of life and economic reproduction—and politics is reduced to the function of "household administration," a function fulfilled by the state (pp. 45–47).

For Habermas, modernity is essentially an epoch of rationalization, as Weber described it. Yet this rationalization is one-sided. The universalization of instrumental rationality—its application to all spheres of social life—converts practical/political questions into technical ones (Habermas 1970, 96). The result is the destruction of the space of democratic decision making, a realm now "colonized" by technical–administrative imperatives (Benhabib 1986, chap. 7; Habermas 1985, 1989).

Reacting to these trends and the withdrawal of the political in the modern age, Arendt and Habermas refined their respective conceptions of the public sphere in order to provide rigorous criteria for determining a *genuine* public realm. By insisting upon the distinctiveness of this realm and the kind of interaction that occurs within it, Arendt and Habermas are able to elucidate the necessary minimal conditions for (to use Arendt's phrase) "the sharing of words and deeds"; that is, by theorizing the conditions necessary for uncoerced deliberation and decision amongst diverse equals, Arendt and Habermas provide an ideal of the public sphere that serves to highlight the forms of asymmetry, coercion, violence, and communicative distortion that presently characterize *our* public realm.

I shall examine Arendt's and Habermas' related conceptions of the public realm and postmodern theory's skeptical response to them. Increasingly, poststructuralist, or postmodern, theorists have raised objections to the fundamental presuppositions of the *type* of project Arendt and Habermas are engaged in. I shall examine what I consider to be the three most basic and forceful objections raised by poststructuralism/postmodernism against Arendt and Habermas. The first, originating in the work of Foucault, I shall refer to as the *power* objection, which radically questions the idea(l) of a coercion-free space of deliberation by retheorizing the nature of power in the modern age (Foucault 1977, 1980a, 1980b, 1982). The second objection, the *epistemological*, finds its most forceful formulation in the work of Lyotard, who challenges the very possibility of a unified, consensus-based public realm in an age that has witnessed the death of legitimating metanarratives and the corresponding fragmentation of the discursive realm into irreducibly heterogeneous language games (1984, 1985, 1988). The third objection, the *ontological*, in some ways the most telling, can be derived from the work of Deleuze or the popularization of certain Deleuzian themes in Baudrillard (Baudrillard 1983; Baudrillard 1988, chaps. 5, 7; Deleuze 1980). This objection questions the peculiar reality attributed to the public realm as a "common space of appearances" (Arendt), a *world* without transcendental or metaphysical support yet available to all its citizens/inhabitants. From the point of view of Deleuze or Baudrillard, the Arendtian conviction that "appearance . . . constitutes reality" betrays a "nostalgia for reality," for a world well lost (Arendt 1957, 57; Baudrillard 1988, chap. 7). This nostalgia appears to permeate Arendt's entire project (the analysis of modernity in terms of "worldlessness"), and Habermas is able to evade it only by offering an abstractly procedural account of discursive rationality.

While these objections are powerful, I believe they are also answerable, at least from the point of view of Arendt's project. Indeed, I will argue that the apparently clear opposition between Arendt and Habermas on the one hand and poststructuralism/postmodernism on the other has been overdrawn thanks, in large part, to the polemical stance taken by Habermas in *The Philosophical Discourse of Modernity* (1987). In that work, Habermas creates a stark opposition between communicative and subject-centered reason, between the paradigms of mutual understanding or intersubjectivity and that of the philosophy of consciousness (chap. 11). In his view, neither Hegel nor Marx succeeded in extricating themselves from the "horizon of the self-reference of the knowing and acting subject"; but then, neither do such critics of the philosophy of consciousness and the modern project as Heidegger, Derrida, and Foucault. All remain, from the Habermasian perspective, either caught up in the metaphysics of subjectivity (Hegel and Marx with their demiurgic conceptions of self-externalizing subjects) or endlessly tracing the transcendental/empirical bounds of the "humanist" paradigm (Heidegger, Derrida, Foucault). According to Habermas, the postmoderns, like their predecessors, *fail to effect the transition to* "*the paradigm of mutual* understanding" and remain locked within an exhausted episteme (pp. 295–96, 310).

This mapping of the world of modern/postmodern theory neatly locates Arendt's work on the intersubjective side of the divide. I shall question the adequacy of this mapping, suggesting that the Arendtian theorization of the public realm harmonizes in unexpected ways with the critics of the strong normative conception of the public sphere. This invites a rethinking of the relation between public realm theory and postmodernism/poststructuralism (see my earlier discussion, Villa 1992).

ARENDT, HABERMAS, AND THE NORMATIVE CONCEPTION OF THE PUBLIC SPHERE

In his appreciation of Hannah Arendt's contribution to the theorization of power, Habermas emphasizes how she "dissociates the concept of power from the teleological model of action" (1983, 173). Contra Max Weber, who "defined power as the possibility of forcing one's own will, whatever it maybe, on the conduct of others," Habermas argues that Arendt "understands power as the capacity to agree in uncoerced communication on some community action" (p. 171). Habermas is referring to Arendt's "On Violence," where she draws a rigorous distinction between *power,* (defined as "the human ability not just to act, but to act in concert") and strength, force, and violence—all of which are uniquely instrumental (Arendt 1968b, 143). This definition points to a model of action quite different from the teleological one, whose means–end structure makes the achievement of a pregiven goal the organizing principle of all action. In contrast, Arendt understands action as "the sharing of words and deeds" among equals. In Habermas' formulation, action is essentially *communicative.* On *this* model—the communicative versus the teleological or instrumental—"the parties are oriented toward agreement and not just toward their respective success" (1983, 173). *Political* power—the power to act together, to act in concert—is generated through the common action of individuals whose interaction is structured in accordance with norms of reciprocity, respect, and equality and for whom agreement or consensus stands "as an end in itself for all parties" (p. 175).

This reading of Arendt's theory of action and concept of power stresses the parallels between her reassertion of the Aristotelian distinction between praxis and *poiesis* and Habermas' fundamental distinction between communicative and instrumental action. It focuses upon the dialogical dimension of Arendt's understanding of politics and asks us to view her notion of the public sphere as one that sets the basic conditions for what Habermas refers to as "coercion-free communication." This is important; for unless political action as communication *is* coercion-free, the consensus that issues from it will have no binding force: it will simply be another expression of insidious or overt violence. Habermas sees the delineation of the public sphere presented in Arendt's *Human Condition* as a phenomenologically inspired attempt to "read off the general structures of unimpaired intersubjectivity in the formal properties of communicative action or *praxis*" (Habermas 1983, 175). Unsurprisingly, Habermas interprets Arendt's conception of the public realm as a prefiguration of his own "ideal speech situation"—the counterfactual elucidation of the formal conditions of coercion-free communication, abstracted (so he claims) from the very structure of communicative action (1987, 315). Theory functions, in both cases, to uncover the gap between current practice and the implicit telos of communicative interaction, the reaching of a "rationally motivated agreement" (p. 315).

While I shall argue that the "consensus" model of politics Habermas attributes to Arendt is problematic, one must acknowledge that Habermas does capture an important dimension of Arendt's theorization of the public realm, as I have argued elsewhere (Villa 1992). In *The Human Condition* and *On Revolution* Arendt consistently emphasizes the idea of the public realm as a space of "tangible freedom"—a space that is physically separate from those of work and labor, bounded or constituted by law, and that can serve as an arena or stage for political action. Not only coercion and violence but all relations of hierarchy or command are excluded from this sphere. The public realm is the space of freedom precisely because it creates an artificial *equality* among persons whose individual talents make them naturally unequal and because it preserves the human condition of *plurality* by relating and separating persons simultaneously. The public realm is that artificial "world" common to us as citizens, an "in-between" that makes both equality and individuality possible (1957, 7, 26, 52; 1963, 31–32).

Here we encounter the seemingly irreducible *spatiality* of the public realm as conceived by Arendt. Unless this sphere has the power to gather us together, to remove each of us from the private while simultaneously guaranteeing each of us a distinct *place* in this "common world," genuine politics is not possible. Where this sphere is invaded by matters and ends that are not essentially open to debate, plurality is effectively neutered and the public realm loses its three-dimensionality: the "world" ceases to be constituted from a variety of perspectives (1957, 55; see Villa 1992, 286–87). Where this space is forcibly collapsed or eliminated (as Arendt argues it is under totalitarianism), the "boundaries and channels of communication between individual men" are replaced by "a band of iron which holds them so tightly together that it is as though their plurality had disappeared into One Man of gigantic dimensions" (Arendt 1968c, 164). The space between individuals is destroyed, with the result that action is no longer possible. So long as the spatial dimension of the public realm is preserved and the "boundaries and channels of communication" remain intact, freedom in the distinctively Arendtian sense is possible.

Richard Bernstein has argued that Arendt's depiction of the public realm in terms of plurality and equality results in a normative conception of politics that is virtually indistinguishable from that of Habermas (1983, 182–223; 1986, 221–37). Plurality and equality open a space of deliberation oriented toward mutual agreement in which the "force of the better argument" (Habermas) can carry the day. Bernstein glosses Arendt's claim that "debate is the very essence of political life" by offering, as a characterization of what happens in the Arendtian public space, that politics

> involves "no rule," the mutual and joint action grounded in human plurality and the isonomy of citizens where individuals debate and attempt to persuade each other. Persuasion, not force or violence, is the quintessence of political life. Persuasion is not the manipulation of others by image-making. Persuasion involves free, open debate among equals in which we seek to form, clarify and test opinions. (1986, 224)

This gloss has the effect of identifying Arendt's public realm with an arena of communicative rationality in which opinion is gradually purified through the "non-coercively unifying, consensus-building force of a discourse in which participants overcome their at first subjectively biased views in favor of a rationally motivated agreement" (Habermas 1987, 315). While Arendt formally eschews Habermas' moral cognitivism, Bernstein points out that this reading finds support in her Kantian theory of "impartial" political judgment (1983, 216–23; 1986, 228–31).

If Bernstein reveals a fundamental continuity between the Arendtian and Habermasian conceptions of the public realm, Seyla Benhabib reinforces that assimilation by arguing that Arendt's concept of plurality must be seen in opposition to the Hegelian–Marxian model of a "supersubject" externalizing and reappropriating itself, a model that reduces plurality to transsubjectivity (1986, 243–44). The phenomenon of plurality, the basic constitutive condition of politics, is covered over by the "work model of action"—Hegel's and Marx's version of the traditional teleological concept. By emphasizing intersubjectivity and the communicative conception of action, the phenomenological ground of politics is recovered: "The communicative model of action does justice to the experiences of human plurality, . . . for communication is the medium through which plurality is revealed. In acting and speaking we show who we are and our differences from each other" (p. 244). The public realm, understood as a discursive space characterized by symmetry, nonhierarchy and reciprocity, both presupposes, and makes possible, plurality and so provides the opportunity for a politics based on mutual recognition and respect for difference.[1]

Postmodern Objections to the Normative Conception of the Public Realm

The account of public realm theory so far offered dwells on the similarities between Arendt and Habermas. In turning to postmodern objections to this type of theory, I shall continue to ignore, for the time being, the substantial differences between them.

Foucault's work can be understood as a sustained critique of public realm theory and its normative presuppositions. As has been widely noted, Foucault's theorization of modern disciplinary power begins with what Nancy Fraser has called a "bracketing of the problematic of legitimacy" (1989, 19; Taylor 1984). By setting aside the fundamental liberal distinction between legitimate and illegitimate power, Foucault brings to light the "local, continuous, productive, capillary and exhaustive" character of modern power (Fraser 1989, 32; see Foucault 1980b, 93–108, 119). Power's "tightly knit grid of material coercions"—its various microtechniques for the production of docile bodies and self-surveilling subjects—remain invisible so long as we treat it as sovereignty (Foucault 1980b, 105). Yet it is just this anachronistic model of state power based on contract and intended for the preservation of rights that liberal theory asserts again and again by its imposition of the legitimacy problematic. In so doing, the liberal paradigm—the "juridical apparatus"—conceals the increasingly insidious forms of modern discipline and domination (Foucault 1980a, 81–91).

Neither Habermas nor Arendt relies upon the discourse of rights to make their critical points, nor can either be accused of holding a "sovereign model" of power. Both point to the possibility of a positive—as opposed to a merely negative or repressive—power, a power that arises from communicative interaction, from "acting together, acting in concert" (Arendt 1968b, 143; 1968c, 172).[2] The negative or repressive aspects are located elsewhere, and identified with strength, violence, hierarchy, and so on. However, from the Foucauldian point of view, this positive conception of power is too clean. It is anachronistic, a democratic analogue to the state-centered liberal model. Both models of power—the positive one proposed by public realm theory and the negative one upheld by liberalism—blind us to the constitutive workings of modern power and its fundamental role in the production of subjects. Thus, the liberal "bounding" of negative/repressive state power through the notion of rights finds its mirror image in public realm theory's specification of the necessary formal conditions for "unforced agreement." In other words, the public realm theory of Arendt and Habermas simply shifts the "power-free zone" of liberalism (power understood as legitimate coercion) from the private realm to the public, redefining power now as to the "ability to act." Indeed, insofar as the public realm is purified of subjective/strategic interests, it is more rigorously coercion-free than the "power-free" private sphere of liberalism. In both conceptions, however, the error is to pretend that we can use power without its using us.

The severe limitations of public realm theory in coming to grips with the nature of modern power are most apparent in its naive reliance upon conditions of symmetry, nonhierarchy, and reciprocity as adequate guarantees of a "coercion-free" space. As Fraser points out, in a "fully 'panopticized' society, hierarchial, asymmetrical domination of some persons by others would have become superfluous; all would surveil and police themselves" (1989, 49; see Dews 1987, 157, 160–61). Foucault enables us to see how hierarchy and asymmetry (the two primary villains according to public realm theory) are not essential to the smooth functioning of disciplinary power. The formation of autonomous individuals—their subjectification—is not so easily detached from their subjugation (Foucault 1977, 29–30; 1980b, 98). Thus, while it is possible to imagine the realization of the ideal speech situation, this would be nothing more than the achievement of "pseudoautonomy in the conditions of pseudosymmetry" (Fraser 1989, 49).[3] A Foucauldian would therefore insist that public realm theory deal with the normalizing character of communicative action. The criterion of consensus is too blunt an instrument for this task, since it leaves unexamined the self-surveillance of the civically virtuous citizen (who has internalized the hegemonic conception of the public good) or communicatively rational agent (who has internalized the hegemonic conception of what constitutes "the better argument").[4]

The second type of postmodernist objection to the normative conception of the public sphere also focuses upon the ideal of consensus—but from an epistemological, as opposed to a power, perspective. Habermas' strategy for rescuing practical/political questions from technocratic usurpation—for preserving the public realm from system colonization—can be reduced to two basic theoretical moves. The first, which draws on Arendt's distinction between acting and making, involves distinguishing between two types of rationalization in modernity: system and communicative rationalization (Habermas 1970, 92–97; Habermas 1985; Wellmer 1985, 35–66). This distinction enables us to view the "disenchantment" of practical discourse in the modern age as a process separate from, yet parallel to, the growth of purposive rationality: *communicative* rationalization consists in the growing endorsement of the principle that validity claims can and ought to be redeemed only through the medium of reflexive speech. The reference to traditional grounds of validation, which presume a prediscursive legitimacy, is no longer possible.

The appeal to communicative rationality (to legitimation through open dialogue and consensus) works, however, only if Habermas is able to convince us that this rationality is universal (and so able to guarantee the possibility of reaching a *genuine* agreement) yet nonsubstantive. Otherwise, he is open to the Nietzschean/Weberian objection that any noninstrumental concept of reason is dogmatic. Habermas' deployment of what he believes is a philosophically viable procedural account of rationality enables him (so he claims) to take full account of the "disenchantment of the world" and the modern separation of value spheres, while avoiding the Nietzschean or Weberian conclusion that will or decision is the only possible source of meaning in a disillusioned age (Benhabib 1986, 258–263; Habermas 1973, chap. 7). A proceduralist account of discursive rationality enables escape from the polytheism of values—Weber's "warring gods"—that characterizes postmodernity. As long as there are standards of argumentative rationality implicit in speech, we will be able to distinguish a genuine consensus from a false one and decisions based on reason from those based upon will or coercion—right from might.

Lyotard, in particular, is sympathetic to the Habermasian attempt to escape the universalization of functionalist reason, the legitimation of knowledge by power. Indeed, his analysis of the principle of "performativity" in *The Postmodern Condition* mirrors much of the standard Frankfurt School critique (1984, xxiv, 47). Nevertheless, he is extremely skeptical of Habermas' attempt to transfer formal rules for what constitutes a genuine consensus from the scientific or theoretical community to the sphere of politics (p. 30).

Lyotard is, first, dubious about the Habermasian picture of the legitimation of scientific discourse (the privileged model of argumentative rationality). "Paralogy," or "permanent revolution," is opposed to consensus (1984, 60–67; Rorty 1985). Second, and more important for my purposes, he points out how Habermas' attempt to avoid an irreversible fragmentation of the public sphere by providing a procedural mechanism that mediates between the "warring gods" and specifies the formal properties of the "better argument" is, in fact, parasitic upon an Enlightenment metanarrative of emancipation. This metanarrative, Lyotard asserts, *was* capable of integrating the language games of science and politics through its deployment of consensus via open debate as *the* criterion of legitimate knowledge and institutions (1984, 32, xxiv). Yet once this metanarrative comes under suspicion (and, as Lyotard famously defines it, postmodernity *is* "incredulity toward metanarratives"), the idea that a model of argumentative rationality and agreement derived from science can provide context-independent criteria of validity is itself cast in doubt.

The Habermasian end run around decisionism and the "polytheism of values" depends upon the availability of a metadiscourse that could, potentially, render the "heteromorphorous" language games of social life—each with its own context-dependent rules of legitimation, competence, and application—commensurable. Against the assumption that "discourse" holds the key to identifying context-independent criteria of validity, Lyotard states that "there is no reason to think that it would be possible to determine metaprescriptives common to all of these language games or that a reasonable consensus like the one in force at a given moment in the scientific community could embrace the totality of metaprescriptions regulating the totality of statements circulating in the social collectivity" (1984, 47; see also 1985, 58). Habermas cannot conceive of legitimation in terms other than a "search for universal consensus." As a result, he *must* assume "that it is possible for all speakers to come to agreement on which rules or metaprescriptions are universally valid for language games" (Lyotard 1984, 47).

For Lyotard, this assumption reduces the irreducible heterogeneity of language games. It is an attempt to neutralize the "diversity of discursive species," each subject to its own set of pragmatic rules. Moreover, Habermas assumes that "the goal of dialogue is consensus," that "humanity as a collective (universal) subject seeks its common emancipation through the regularization of 'moves' permitted in all language games, and that the legitimacy of any statement resides in its contributing to that emancipation" (ibid., 65–66). *This* assumption not only does violence to the heterogeneity of language games, it does violence to the heterogeneity of the players—their plurality—by subjecting all to a regime of discursive practice designed to overcome what Lyotard sees as the *agonistic* dimension of those games (p. 10). The spontaneity, initiation, and difference that characterize agonistic speech are repressed as a model of Kuhnian "normal science" is imposed upon the public sphere. Because of this flattening, antiagonistic, antiinitiatory character of the consensus model, Lyotard concludes that "consensus has become an outmoded and suspect value" and opts instead for a "pagan" politics that breaks decisively with all attempts to ground action and practical decision in a theoretical discourse of justice or legitimacy (1984, 66; 1985, 19–43, 28).

From Lyotard's perspective, the fragmentation of the public sphere—the emergence of discordant language games that results from the postmodern death of totalizing metanarratives—opens the way to forms of political practice and judgment freed from the tyranny of science or *episteme* (1984, 28–29; 1985, 27, 58). "Paganism"—Lyotard's code word for an antifoundationalist politics of opinion, of judgment "without criteria"—rescues the political by unmasking the ideal of a grounded consensus and the theoreticist/Platonist constraints it implies. Lyotard's epistemological rejoinder to the Habermasian construction of the public sphere parallels Foucault's objection in its bracketing of the legitimation problematic. (Lyotard sees this problematic as encompassing any attempt to derive the political from the theoretical, the just from the true, even where "truth" is seen as a function of consensus; see 1985, 25.)[5] Public realm theory conceals the disciplinary underside of "acting together" and covers over the antiagonistic, antiinitiatory implications that flow from the regularization of moves in any language game.

The final objection to public realm theory raised by postmodernism is ontological, so-called because it is directed against the specific reality claimed for the public sphere and the objects within it. It is more directly applicable to Arendt's theory, which presents us with what could be called an ontology of the public realm. Eschewing Habermas' attempt to delineate the public sphere in terms of a specific form of interaction and the rationality appropriate to it, Arendt defines the public phenomenologically, as a "common space of appearances." Plurality has a constitutive, as well as a consensual, dimension. Deliberation and debate from a diversity of perspectives constitutes a "shared world of appearance," a world whose reality is a function of incessant speech about that which lies between us (Arendt 1957, 50–58). From Arendt's point of view, that which "appears in public and can be seen and heard by everybody" is "real." This conviction is expressed in her dictum that "for us, appearance—something that is being seen and heard by others as well as ourselves—constitutes reality" (p. 50).

In making this claim, Arendt is self-consciously aligning herself with Nietzsche's undermining of the Platonic metaphysical distinction between appearance and reality (1977, vol. 1, 10). But where Nietzsche sees this distinction as expressing an ascetic devaluation of life, Arendt sees it as expressing a philosophic hostility to politics. Her desire to "save the phenomena"—her insistence that meaning and reality reside *in* the appearances, not behind or above them—follows Nietzsche's revaluation of the world of becoming (Villa 1992, 287–88). However, Arendt backs away from the conclusion Nietzsche drew when he traced the nihilistic logic set in motion by the institutionalization of the Platonic distinction, the two-thousand-year process by which the "real world becomes a fable": "We have abolished the real world: what world is left? The apparent world perhaps?

. . . But no! with the real world we have abolished the apparent world!" (1979, 41). For Nietzsche, try as we might to divorce it from any transcendental signified, our concept of appearances is irreducibly metaphysical. To presume, as Arendt does, that one can hold onto an "objective," shared world of appearances once all anchors to a transcendental reality are cut loose is, ultimately, to remain entangled in the web of metaphysics.

Arendt believes that the reality of the world, of a common space of appearances, is guaranteed so long as this in-between is the arena for spirited debate and deliberation. Where public spirit and "care for the world" animate a community, Arendt believes, the *sensus communis* remains strong, providing a nontranscendental ground for action and judgment (1957, 208–9, 274–85; 1968a, 221; 1982, 66–72). Such a "common feeling for the world" transcends the local, concretely contextual nature of Aristotelian ethos. At the same time, it makes no claim to providing a cognitive ground for moral/political judgment. Yet this "feeling for the world" is vulnerable, the easy victim of the modern subjectification of the real that Arendt traces in *The Human Condition* (1957, chap. 6, esp. 280–88). Hence the mournful Arendtian leitmotif of modern "worldlessness," the peculiar alienation from the world that accompanies the withering away of "common sense."

Here Deleuze's interpretation of Nietzsche and Baudrillard's theorization of simulacra show us how the "loss of the world" remains trapped within the logocentric/metaphysical logic of the sign. From this perspective, Arendt does not overcome the terms, she merely inverts them. The Arendtian public sphere is a space of genuine appearances, of signs whose play and self-referentiality is restricted by their being "seen and heard by everyone." This implies and preserves an opposition between an authentic, real in-between and its inauthentic, manipulated counterpart—the former both condition and result of action and speech, the latter nothing more than the proliferation of signs/images that only serve further to destroy our feeling for the world. Baudrillard's point is that this way of looking at things (of seeing the "world" as increasingly "dimmed-down" or covered over) does not make a final break with the representational episteme. Postmodernity, on the other hand, has left the "order of appearance" behind: "reality" (including the reality of appearances Arendt wants to preserve) is presently generated as a simulation effect. The "decisive turning point," Baudrillard states, is "the transition from signs which dissimulate something to signs which dissimulate that there is nothing" (1988, 170). Arendt's ontology of the public realm prevents us from seeing just how well and truly lost the world is. Her nostalgia for the "world," for worldliness, is a nostalgia for a common referent—a nostalgia that power is able to exploit for its own ends, in order to "rejuvenate the fiction of the real" (ibid., 178).

REDRAWING THE MAP: RESPONSES AND REPOSITIONINGS

I began by suggesting that the relation between public realm theory and postmodernism/poststructuralism was more complex than Habermas' mapping of it allowed. By starkly opposing the paradigm of the philosophy of the subject and that of mutual understanding, Habermas can present postmodernism/poststructuralism as the coda to an exhausted project, the closure of subject-centered reason, rather than what comes after (1987, 296). Public realm theory stands, in contrast, ready to aid in the positive realization of the modern project, abandoning totalizing critique for the new paradigm.[6]

The danger of this polemical opposition is that it blinds us to the possibility of a different configuration. Once we detach Arendt's conception of the public realm from the problematic that Habermas constructs, a more complex articulation of public realm theory and postmodernism/poststructuralism becomes possible. If we step back from the issues of legitimation and consensus, Arendt's theory reveals a narrative structure that hooks up, in unexpected ways, with the concerns of postmodern/poststructuralist critics.

With respect to Foucault's power objection, one must first acknowledge that Arendt and Foucault are simply talking about different things when they use the word *power*.[7] True, Arendt's related concepts of violence, strength, authority, and so on are scarcely suited to capture the mechanisms that Foucault wants to investigate. Nevertheless, it is obviously inappropriate to presume a common referent here and then to argue that one theorist is right, the other wrong. More important, however, is that Habermas is wrong to treat Arendt's definition of power ("the ability to act together, to act in concert") as implying a consensus-centered conception of the public realm. From Arendt's point of view, plurality is not just a condition, but also an *achievement* of political action and speech: these activities give public expression to difference. The theory of political action presented in *The Human Condition* takes as its ideal an agonistic subjectivity that prizes the opportunity for individualizing action (1957, chaps. 2, 4). Contra Habermas, Arendt views action as an end in itself: its value in no way hinges upon the formation of a rational general will. Like Foucault and Lyotard, she fears that the regularization/rationalization of communicative action leads to the creation of docile subjects.

This concern about the denaturing consequences of a regularized, consensual politics comes across most strongly in Arendt's analysis of postrevolutionary American politics in *On Revolution*. Here she traces how the advent of a representative system transforms the public sphere, substituting voting and judicial decision making for spontaneous, agonistic action (1963, chap. 6). The loss of the "revolutionary spirit" yields a routinized politics of interests and general welfare: *actors* are transformed into *clients* who are more or less effective in persuading a de facto oligarchy to favor their particular interests. The overall effect of the representative system is, from Arendt's perspective, virtually to eradicate the public space for agonistic action and to redirect political or public energies to the pursuit of private or social goals. The result is an essentially passive, depoliticized citizen body.

The regularization of politics (the containment of its spontaneous, popular, and agonistic dimensions) by the mechanism of representation points us to that larger, peculiarly modern threat to the public sphere—the rise of the social. With the emergence of this inclusive, hybrid realm in the modern age, the conditions of possibility for either a genuine public or private sphere are undermined. Moreover, the ubiquitous functionalization that accompanies the rise of the social imposes its own constraints on agonistic political action. As Arendt puts it:

> It is decisive that society, on all its levels, excludes the possibility of action, which formerly was excluded from the household. Instead, society expects from each of its members a certain kind of behavior, imposing innumerable and various rules, all of which tend to "normalize" its members, to make them behave, to exclude spontaneous action or outstanding achievement. (1957, 40)

It is here that Arendt's concerns most nearly intersect with Foucault's. Once we see Arendt's public realm as a space for agonistic action denatured by the normalizing power of the social, we can make suggestive connections to the Foucauldian story about the take-off of disciplinary power in the modern age. Arendt hardly shares Foucault's desire to develop a politics of everyday life. Nevertheless, they are both concerned to tell the story of how an essentially theatrical space (the agonistic public realm of Arendt, the premodern public realm of spectacle in Foucault) is colonized by a new form of disciplinary or "socializing" power, a power that substitutes an institutionally dispersed and normalizing regime of panoptic visibility for a centralized space in which *action* is seen and heard by all. The telos of this transformation, for both Arendt and Foucault, is the better preservation, cultivation, and management of the state's most precious resource, its populace. There is, I would suggest, a direct line to be drawn from Arendt's conception of the state as "national household" to Foucault's notion of biopower (Foucault 1980a, 140–43; 1988).

Foucault and Arendt are linked, moreover, by a concern to preserve forms and spaces of popular, spontaneous action ("counter-power") from encroaching bureaucratic structures. (Compare, for example, Arendt's emphasis on the spontaneous, popular nature of revolutionary action in *On Revolution* to Foucault's defense of direct forms of popular justice in *Power/Knowledge*; see Arendt 1963, chap. 6 and Foucault 1980b, 27–32.) Reading Arendt and Foucault in this manner suggests that Arendt's highly specific, exclusive notion of political action is not *contradicted* by Foucault's account of power. Rather, they present complementary narratives about the closure of the space for action in the modern age. From this standpoint, the Foucauldian concept of "resistance"—of local struggle against power/knowledge regimes—can be seen as a successor concept to Arendt's notion of political action. Where the space for action is usurped—where action in the strict sense is *no longer possible*—resistance becomes the primary vehicle of spontaneity and agonistic subjectivity.[8]

The reading I have just sketched moves Arendt's public realm theory from the legitimation problematic to that of agonistic subjectivity. The "structures of unimpaired intersubjectivity" that Habermas stresses give way to a performance-centered conception of action framed in terms of a narrative about the closure of the public space in the modern period. This displacement reveals similar continuities with Lyotard's project. A consistent theme in Arendt's work is the hostility of truth to politics. At no point in her thought on the nature of political action does she embrace moral cognitivism or indicate a belief in *theoretical* criteria that could distinguish a genuine from an inauthentic consensus. For Habermas, the Arendtian repudiation of the will to truth in politics is evidence of an anachronistic (Aristotelian) conception of theory (1983, 194). This objection, however, surely misses the point. The reason that Arendt sees "a yawning abyss between knowledge and opinion that cannot be closed by rational argument" is that her conception of political action is antiteleological (p. 194). Like Lyotard, she disputes the Habermasian assumption that "the goal of dialogue is consensus." Her account emphasizes action as an end in itself—the *performance,* as opposed to the *product,* of praxis.

Arendt, then, shares Lyotard's generally agonistic conception of speech. For both, "to speak is to fight, in the sense of playing, and speech acts fall within the domain of a general agonistics" (Lyotard 1984, 10; see Arendt 1957, 41–42). Moreover, her emphasis on the initiatory dimension of action ("to act . . . means to take an initiative, to begin") parallels Lyotard's focus upon the speaker's ability and need to create new moves, invent new criteria (Arendt 1957, 177; Lyotard 1985, 61). Arendt views the obsession with forming a rational general will as inevitably normalizing and destructive of plurality. In order to preserve this plurality from the regularizing force of consensus and theory, she endorses a "pagan" politics of opinion, of judgment "without criteria." Lyotard's slogan that there can be no "derivation of the political from the theoretical" captures the spirit of Arendt's own vehement anti-Platonism (Lyotard 1985, 26–27).

But if Lyotard's idea of a politics founded "on the basis of opinion" seems largely derivative of Arendt, there remains one seemingly irreconcilable difference. Lyotard celebrates the fragmentation of the public space; he glories in the heterogeneity this fragmentation produces. The only criteria of legitimacy he recognizes are local and context-specific, first-order narratives, as opposed to metanarratives/metadiscourses. In *Just Gaming*, this position brings him very close to identifying the Aristotelian idea of an ethos as the sine qua non of a politics "without the Idea of Justice" (1985, 26). Arendt, whose own theory of political judgment presents a similar mix of Aristotelian, Kantian, and Nietzschean elements, avoids the relativist consequences of postmodern pluralism by appealing to the Kantian idea of a *sensus communis*, a common feeling for the world, which provides a nontranscendental basis for judgments making a universal claim of validity (Arendt 1968a, 220–21; see Villa 1992, 301–2). Lyotard views the appeal to the *sensus communis* as an act of bad faith: the withering away of common sense in postmodernity means, simply, that "there cannot be a *sensus communis*" (1985, 14). According to Lyotard, there is no escaping the fact that *we* "are in the position of Aristotle's prudent individual, who makes judgments about the just and the unjust without the least criterion" (ibid.).

As in the case of Foucault, the opposition here is less dramatic than it first appears. Arendt does not dogmatically assert the existence of "a common feeling for the world" in a last-ditch effort to avoid acknowledging the inescapable fragmentation of the public sphere. If, as with Foucault, we see Arendt as essentially providing a narrative about modernity, then we also can see that she hardly disputes the postmodern assertion that reliance upon the *sensus communis* is no longer possible. The last part of *The Human Condition* is an extended meditation on how the energies of modernity have worked to dissipate our feeling for the world. The "worldliness" presupposed by the *sensus communis* is *not* a distinguishing characteristic of the *animal laborans* and the late modern polity. Indeed, Arendt would go further than Lyotard on this score: the "blurring of boundaries" effected by the modern rise of the social destroys the integrity of language games and local contexts. The ground necessary for anything approximating an ethos is lacking (Arendt 1957, chap. 2, sec. 6 and chap. 3, sec. 17).

This brings us, finally, to the ontological objection. If, as I have suggested, Arendt's public realm theory shares the Foucauldian/Lyotardian concern with the preservation of agonistic subjectivity—if we see her as calling for a politics that stresses plurality, difference, spontaneity, and initiation against the regularizing apparatus of consensus—*then* the relation between her theory and poststructuralism/postmodernism is quite different from that suggested by the Habermasian mapping. Can a similar "shift" be effected with respect to the ontological objection? I would answer *no*: here we confront *the* insurmountable difference between Arendt's public realm theory and postmodernism. This difference is *not*, as some might suggest, the result of a misplaced foundationalism or realism on Arendt's part. Rather, it is a question of profoundly different attitudes toward a single phenomenon, the "de-worlding of the world" (to use Heidegger's phrase). Arendt mourns what the postmodern celebrates, the loss of certain ontological dimensions of human existence (action, the shared public world, the self as performance). The postmodern blurring of boundaries, the effacement of any meaningful distinction between the authentic and the inauthentic—reality and its simulacra—renders the Arendtian idea of the phenomenal integrity of a distinctly *political* realm simply nostalgic.[9] Arendt chooses, if not to *recover* the public realm, at least to preserve its memory. The "affirmative nihilists" of postmodernity wish to have done with such guilty nostalgia, viewing it as one more symptom of essentialism. There *is* a choice here, but it is not the one offered by Habermas. It is the choice between a politics of mourning and a politics of parody, a politics that remembers the *res publica* and a politics engaged in the endless subversion of codes.[10]

Notes

I thank Bill Connolly, Fred Dolan, Bob Gooding-Williams, Tom Dumm, and Austin Sarat for their comments and criticisms.

1. In a recent essay, Benhabib (1992) contrasts Arendt's model of an agonistic public space to Habermas' "discursive" model. Having previously argued that Habermas' theory of communication does justice to the phenomenon of plurality (Benhabib 1986), she attempts to show how the agonistic dimensions of Arendt's conception presuppose untenable distinctions (e.g., between the social and political) and exclusions. The upshot of both treatments is that Habermas absorbs what is valuable in Arendt's public realm theory, while avoiding its more "anachronistic" (Greek) features. In contrast, I believe Habermas has radically foreshortened the phenomenon of plurality and that the expression of this phenomenon demands something like Arendt's agonistic conception of political action. Cf. Connolly 1991.

2. In this regard, see the interview between Foucault and Rabinow, Charles Taylor, Martin Jay, and Richard Rorty (Foucault 1984, 373–80). Foucault is asked to comment upon the "Arendtian/Habermasian" conception of power (p. 377). His response is minimal: he admits the need to hold onto the notion of "consensual politics" as a regulatory principle, a statement that echoes his view in "The Subject and Power" (1982), where he takes a stand against nonconsensual power relations while remaining wary of the concept of consensus *überhaupt*. He fudges the question with specific regard to Arendt by stating: "It seems to me that in many of the analyses that have been made by Arendt, or in any case from her perspective, the relation of domination has been constantly dissociated from the relation of power. Yet I wonder whether this distinction is not something of a verbal one" (p. 378).

3. It should be added that Fraser rejects this possibility.

4. Hence, Foucault's ambivalent or ironic attitude toward the notion of consensuality. See n. 2. See Dumm 1988.

5. This bracketing leads Seyla Benhabib to mount a criticism of Lyotard parallel to Fraser's critique of Foucault. See Benhabib 1984, 114–15.

6. In fairness to Habermas, it should be noted that this mapping (at least as regards public realm theory) has been largely accepted by poststructuralist/postmodernist critics.

7. Janicaud (1992) points out that the critique Habermas offers of Foucault's theory of power substitutes the German *macht* for the French *pouvoir*, thereby eliding the distinction between *puissance* (strength) and *pouvoir* (power). This elision, similarly found in the English translation, facilitates the critique of Foucault as "normatively confused," a charge made by Habermas (1987), Fraser (1989), and Taylor (1984). But, Janicaud points out, *pouvoir* is typically reserved for "specific juridical-political forms and moralities" (1992, 284). Thus, while Foucault and Arendt use *power* to designate two

different phenomena (the juridical–disciplinary apparatus and concerted popular action), there is hardly the generalized conflation of power, strength, violence, and so on in Foucault that some of his critics charge. As Foucault makes clear in a number of places (esp. 1982, 220), he is sensitive to the kind of distinctions Arendt makes in "On Violence"; indeed, he considers them essential. The fact that he prefers to call popular action, à la Arendt, "counter-power" needs to understood against the background of his thesis concerning the ubiquitous, capillary, and productive nature of disciplinary power.

8. I should note that for Arendt, resistance *is* a form of action (1968a, pref.).

9. There is an obvious parallel here to Derrida's charge that Heidegger, while deconstructing the metaphysics of presence, nevertheless betrays a nostalgia for presence in his employment of the metaphorics of nearness and farness (of Being). See Derrida 1976, 22, 143; Derrida 1980, 130, 133. For a response to Derrida's critique, see White 1991, 65.

10. Fraser (1990) offers a critique of the Habermasian public sphere along quasi-Lyotardian lines, one that suggests an alternative beyond the purely mournful or subversive attitudes I have sketched. Arguing that the accordance of normative status to a singular, comprehensive public sphere enables domination via discursive means, Fraser opts for a more flexible theorization of the public sphere that makes room for a plurality of competing publics. This move from a strong normative conception to a more phenomenological account has the virtue of expanding our view of available and politically relevant discursive spaces by including "subaltern counterpublics" as crucial alternative forms of publicity. Fraser believes that participatory parity is better achieved by the cultivation of multiple publics that interact agonistically, rather than by the attempt to encompass all within one deliberative paradigm. Whether or not one finds Fraser's attempt to split the difference between postmodernism and public realm theory persuasive, it does serve to highlight one important consequence of Arendt's shift away from the legitimation problematic. Once this shift is made, the idea of the "public sphere" ceases to function in an essentially normative capacity. No longer setting the standards for genuine agreement or a just being-together (except in a minimal way, e.g., the prohibition of force and violence), public realm theory, in Arendt's hands, transforms itself into narrative, telling a story that keeps alive the memory of an agonistic public realm. If we overcome Arendt's nostalgia for an integral public realm, narratives such as those offered in *The Human Condition* and *On Revolution* can be seen as enabling us to identify, and see as significant, practices and spaces that would otherwise be overlooked: wherever we find agonistic spontaneity, resistance, and theatricality, there we find a *political* practice. The result is the cultivation of an agonistic political ethos that avoids claiming more for "subversive practices" than these practices can bear but that also eschews the alternative of mournful withdrawal.

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RESEARCH
NOTES

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SOCIOECONOMIC CLASS BIAS IN TURNOUT, 1964–1988: THE VOTERS REMAIN THE SAME

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We address the question of whether class bias in the American electorate has increased since 1964. We analyze the Census Bureau's Current Population Survey and the National Election Studies for seven consecutive presidential elections, 1964–88. Our results show that conclusions regarding changes in class bias are sensitive to which measure of socioeconomic class is used—income, education, or occupation. We argue that income is the appropriate measure since government policies that discriminate based on socioeconomic class are most likely to do so based on income and there are measurement problems associated with using either education or occupation over time. Our analysis shows that there has been almost no change in class bias in the electorate since 1964.

Who votes? has endured as a central question throughout several decades of electoral research. Much of this research has focused primarily on the demographic correlates of voter turnout. The common wisdom that education, income, and age are positively associated with turnout in presidential and off-year elections has been demonstrated most persuasively by Wolfinger and Rosenstone (1980) and supported by a score of other analyses (Cassel and Hill 1981; Conway 1981; Teixeira 1987; see also our work, Leighley and Nagler 1992).

While these analyses imply that voters are of higher socioeconomic status than nonvoters, the magnitude of the difference and, more importantly, its stability over time are disputed. Burnham (1982, 1987a), Bennett (1991), and Reiter (1979) argue that turnout decline since the 1960s has been significantly greater among low-status groups than high-status groups, while both Cavanagh (1981/82) and Teixeira (1987), using different measures and different time periods, claim otherwise. Any claim of disproportionate decline in turnout among one class of the potential electorate has troubling normative implications: if a predominantly upper-class actual electorate emerged from the low level of turnout, then we would expect policies chosen by elected officials to reflect such an elite bias; and representational ideals would suffer (Key 1949). Alternatively, had the decline in turnout since 1960 occurred equally across all social classes, then, at least in terms of representational equality, the character of the democratic process would not be altered.[1]

We challenge the conclusion that the post-1960s decline in voter turnout is marked by a large increase in socioeconomic class bias in the electorate. We examine the relative turnout rates of different demographic groups from 1964 through 1988 and demonstrate that there is actually remarkable stability in these relative rates over this period. Using multivariate analysis, we show that the effect of income on turnout has remained stable. In addition, we demonstrate why other scholars have reached a different conclusion regarding the change in class bias in the electorate.

ANALYZING TURNOUT

There are several possible reasons for disagreement regarding changes in the effects of socioeconomic status on turnout. First, observed effects will depend upon the choice of election years. Examining different years may generate conflicting findings for either of two different reasons: the sample estimates may be unstable from year to year, or the true population parameters may change from year to year. Attempts to solve the first problem by pooling the data across elections make the strong assumption that the effects of demographic characteristics on turnout do not change from year to year (see Teixeira 1987). If this assumption is incorrect, then, pooling the data sets may solve the problem of unstable coefficients but introduce yet another: the pooled model will be badly misspecified; hence, the resulting estimates will be biased and inconsistent (Johnston 1984).

A second possible reason for differing findings is that analyses of turnout often use reported, rather than the validated, vote. To the extent that misreporting is associated with other hypothesized correlates of turnout, findings based on the reported vote alone may simply be wrong (Hill and Hurley 1984). Katosh and Traugott (1981) and Sigelman (1982) assert that most inferences based on reported vote will remain true when the validated vote is examined. However, Abramson and a number of colleagues have shown quite persuasively that blacks are more likely than whites to overreport (Abramson and Claggett 1984, 1986; Silver, Anderson and Abramson 1986; Anderson and Silver 1986). Since race and income are correlated, inferences about the effect of income on turnout based on the reported vote may be biased, as well.

Third, disparate findings may arise from the choice and measurement of variables included in multivariate models. Since many of the demographic factors commonly included in studies of turnout are highly correlated (e.g., race, income, and education), the estimated effects of each factor will be extremely sensitive to the inclusion or omission of other factors (Johnston 1984). For instance, including occupation as a variable will alter estimates of the effects of income, education, and sex on turnout. Thus, comparison of findings regarding the latter three items across studies that are not consistent with regard to the inclusion of occupation becomes impossible.

Finally, conclusions regarding the representation of the socioeconomic groups in the electorate will necessarily be affected by the validity of the measurement of socioeconomic status. As shown by Wolfinger and Rosenstone (1980), the three standard indicators of socioeconomic status are related to turnout in different ways. This suggests that conclusions regarding socioeconomic class bias may be sensitive to which measure of socioeconomic status is chosen. Much of our analysis is devoted to demonstrating this point.

THE TURNOUT DATA

We analyze voting turnout in the presidential elections of 1964 through 1988 using the Current Population Surveys (CPSs) from the Bureau of the Census, as well as the American National Election Studies (NES) of the same years. We begin in 1964 because it marks the beginning of the long-term post-1960 decline in presidential turnout. Thus, we are able to examine whether that aggregate turnout decline has been distributed unevenly among different socioeconomic groups. Since 1964 is also the base year used in Burnham's (1987a) calculations, we can compare our findings to his. The CPS is a monthly survey of individuals conducted by the Bureau of the Census. Each November of even-numbered years the survey includes a set of questions about individual voting behavior, as well as the standard battery of monthly questions about respondents' income, education, occupation, and other demographic characteristics. Because the original data from the 1964 and 1968 CPS surveys are no longer available, our analysis for those years is limited to information available in Census Bureau reports. For the 1972–88 elections, we are able to provide a significantly richer analysis of changes in the electorate.

The CPS data offer several advantages in analyzing turnout in presidential elections due to sample sizes. In each election year there are from 89,000 to 115,000 valid respondents, approximately 50 times the number of respondents available in comparable NES data sets. This means that estimates of population parameters are much more precise. We are able to determine whether year-to-year variations in parameter estimates are the result of actual changes in the true parameters (i.e., changes in the real world), rather than simply being artifacts of the limited precision of sample estimates. While the NES data allow for estimating the effects of highly correlated measures, the limited sample size precludes firm conclusions regarding the *stability* of the population parameters from year to year.[2]

We compute turnout from the CPS as the percentage of respondents who report that they voted in the recent presidential election relative to respondents who provide valid responses to the question. This is slightly different from the Census Bureau's procedure.[3] The Census Bureau treats individuals with no response to the vote question as nonvoters for the purposes of their turnout calculation. Hence, our estimates of reported turnout are consistently higher than the Census Bureau figures.[4]

Since the NES is the standard source for studies of voter behavior, we compare our central findings to similar analyses of the NES data. In addition, we compare our findings based on the reported census vote to an analysis based on the NES validated vote for each election from 1964 through 1988, excluding 1968 and 1972.[5]

In the analyses that follow, we analyze class bias in six ways: (1) we report turnout rates for different socioeconomic groups; (2) we plot the relative turnout rates for different income groups over time; (3) we compute modified Gini coefficients based on income quintiles to obtain income-based measures of class bias that are based on the entire population, rather than the extremes of the income distribution; (4) we compare the turnout gap across all three types of measures (income, occupation, and education) to assess the sensitivity of findings to the choice of measure; (5) we analyze misreporting rates across class groups to test the robustness of our findings based on the reported vote; (6) we estimate a multivariate model using both the reported vote and the validated vote, to test for stability in the marginal effect of each measure of class.

TURNOUT AND SOCIOECONOMIC STATUS

Exactly how the socioeconomic composition of the electorate might have changed between 1964 and 1988 is not obvious. Examination of Census Bureau data shows that the percentage of high school graduates in the bottom income group rose 29.2% from 1964 to 1988, compared to only 16.5% for the top income group. Similarly, the percentage of high school graduates among blue-collar workers and white-collar workers increased by 35.4% and 14.3%, respectively. Since increases in levels of education have occurred more among the lower socioeconomic groups, we would expect the participation gap between the lower and upper socioeconomic groups to *decrease*. Alternatively, the poor were more likely to be young and single and to reside in female-headed households in 1988 than they were in 1964. We would expect the first two of these changes to lead to lower voting rates among the lower-income group, that is, ceteris paribus, a *larger* participation gap in 1988 than in 1964.

The last, in fact, is the claim that Burnham makes: "The post-1960 turnout decline has been concentrated among those who had already participated least in the 1940–60 period" and is "strikingly selective" (1987a, 124–5). Burnham argues that after 1968, the low-cost information supplied by partisanship for voters became irrelevant in the new image-based politics. According to Burnham, the electoral consequences of these changes have not been equal across social classes because the participation of low-status individuals is more heavily dependent upon political institutions (e.g., party mobilization) than on personal resources.

Burnham (1987a) provides evidence for this thesis using data on working- and middle-class turnout in presidential elections in Boston from 1964 through 1984 and census data on the turnout of various occupational groups at two points in time. Census data for 1964 and 1980 show that the turnout rate of men classified as manual labor (blue-collar) dropped from 66.1% in 1964 to 48% in 1980. During this same period the rate for white-collar men dropped from 83.2% to 73.0%. Thus, while blue-collar male turnout dropped 18.1%, white-collar male turnout dropped only 10.2%. This differential decline is what Burnham refers to as "selective demobilization."

Reiter (1979) also claims that the class bias in turnout has increased. Rather than using occupation as a measure of class, he compares income quartiles for white NES respondents from 1960 through 1976. According to these figures, the gap between the voting rate of the top and bottom income quartiles has gone from 18.2% in 1960 to 28.7% in 1976. However, these findings are restricted to whites and hence may not provide an accurate picture of the entire electorate. This is particularly important because the time span Reiter focused on encompasses the large change in black turnout following the enactment of the 1965 Civil Rights Act. Basing conclusions on only the top and bottom quartiles of the income distribution also ignores possible changes in the turnout pattern of the middle two quartiles.

Bennett (1991) measures class bias in voting by education level and draws the same conclusion as Reiter for *whites under the age of 35*: class bias has increased. He reports that from 1964 to 1988, turnout among college graduates in this group went from 71% to 59%, while among whites without any college years, turnout dropped from 63% to 31%.[6] Bennett's findings bear less directly on the question of socioeconomic class bias in the electorate because the distribution of education in the population has changed so much from 1964 to 1988. According to the NES data, in 1964 only 30% of whites under 35 had some college experience. In 1988 the size of this group increased to 45% of whites under age 35.[7] Hence, Bennett is comparing a relatively high-status group in 1964 to a relatively lower-status group in 1988; persons who had attended college were rare in 1964 compared to 1988. Thus, the temporal nature of education as a measure of socioeconomic status can thwart attempts to measure change in the socioeconomic voting gap.

Finally, Cavanagh (1981/82), who reports no net change in class representation since the 1950s, provides evidence contradicting these conclusions. Dividing the NES data into thirds rather than quartiles, he computes a class bias ratio defined as the ratio of the turnout rate of the top third income group to the turnout rate of the bottom third income group. He points out that the years 1960 and 1964 were relative lows in class bias for the postwar period and that in fact the class bias scores for 1972 and 1976 are approximately the same as those for 1952 and 1956. This evidence highlights how critical the choice of years is in drawing inferences regarding changes in socioeconomic class bias. It is also interesting to note that the time period that Cavanagh describes as being marked by unusually low socioeconomic class bias includes the election year (1964) that Burnham uses as a base for his calculations.

That Cavanagh's claim of no change in class bias is based on an income measure suggests that the choice of socioeconomic status measures may be critical for the conclusions drawn regarding socioeconomic class bias. Burnham (1987a) based his conclusion regarding the 1964–80 period on only one measure of socioeconomic status, occupation.[8] There are three reasons to prefer income to occupation as the relevant measure of socioeconomic status. The first reason is fundamental to the entire exercise; income is the more relevant measure with regard to government policy. Few government programs convey benefits based solely on occupation. Instead, government policies are debated in terms of their effect on the rich versus the poor; and many government policies explicitly distinguish between individuals based on income: the tax code, Medicaid, Aid for Families with Dependent Children, and so on. The second and third reasons are based on the ambiguity in measuring occupational status. Some occupations may be difficult to classify as either white-collar or blue-collar in a given year. Finally, the socioeconomic class rankings of occupations may not be stable over time.

Table 1 shows the overall turnout rate for presidential elections from 1964 through 1988, as well as the turnout rate by different measures of socioeconomic status: income, occupation, and education. Individuals' family income is coded into quintiles, using the overall family income distribution among the potential electorate (i.e., among individuals over the age of 18) for 1964–88. Quintiles of income are used, rather than reported family income for each election year, so that the indicator reflects socioeconomic status instead of absolute wealth. This is especially appropriate for comparisons across time, since an individual's ranking in the income distribution is more comparable over time than is the individual's absolute income level. (See Table A-1 for the exact size of each income quintile.)

TABLE 1

Turnout by Demographic Characteristics (1964–88, Current Population Surveys)

DEMOGRAPHIC GROUP	1964	1968	1972	1976	1980	1984	1988
Total	70.5	70.3	64.9	62.6	62.3	63.1	60.1
Income							
1 (Low)	53.7	57.5	50.7	47.4	47.7	47.8	42.7
2	63.1	67.0	55.9	55.1	54.2	57.5	52.2
3	72.8	74.2	63.3	62.4	60.5	63.5	58.9
4	78.7	79.7	71.6	69.9	68.9	70.3	67.5
5 (High)	85.2	85.4	80.7	78.4	76.1	77.6	76.9
Gini coef.	—	—	.0941	.0984	.0997	.0985	.1112
Occupation							
Service work	65.9	62.7	58.6	52.8	51.3	52.9	47.2
Blue-collar	65.6	62.3	54.2	49.8	48.0	49.4	44.5
White-collar	82.1	79.8	76.4	72.1	70.9	70.8	68.5
Education							
No high school	59.4	56.4	48.9	46.8	44.5	45.1	38.4
Some high school	65.9	63.1	53.7	50.1	48.4	47.0	43.3
High school grad.	76.5	74.2	66.1	62.0	61.0	61.0	56.6
Some college	82.6	80.2	76.0	70.1	69.5	69.9	66.4
College grad.	—[a]	84.7	83.6	81.1	80.1	79.3	75.8
Post-college	88.2	86.9	87.4	85.2	84.9	84.4	83.9

Note: Table entries are the reported turnout rates for each demographic category.
[a]For 1964 *college grad* and *post-college* are only available in combined form.

In any given year, the socioeconomic class bias in turnout is clear, with the highest-income group typically voting at over 70%, and the lowest-income group typically voting close to 50%. However, the wide range in turnout across income groups does not imply a significant change in *relative* turnout rates over time. From 1964 to 1988, turnout of the lowest-income group dropped 11%, while turnout for the highest-income group dropped 8.3%, suggesting only a slight increase in socioeconomic class bias.

If we consider the time period 1964–80 (used by Burnham 1987a), we find that turnout dropped 6% for the lowest-income group and 9.1% for the highest-income group. This suggests that based on income, class bias actually *decreased*. These findings highlight how critical the choices of time period and measures of socioeconomic status are to conclusions regarding class bias (points to which we shall return shortly). Moreover, they provide preliminary evidence that Burnham's conclusions regarding class bias across the 1964–80 period are largely a function of his choosing occupation as a measure of socioeconomic status.

Figure 1 graphs the difference between the turnout rate of each income group and the overall turnout rate for 1964–88. This provides a measure of the changes in each income group's turnout relative to changes in the overall level of turnout for each year. Disproportionate decline in the turnout of one class would be reflected in the difference between that class's turnout and overall turnout becoming increasingly negative over time. The larger the negative turnout difference, the greater the underrepresentation of that class. Thus, if a group's representation among voters is declining, as Burnham (1987a) claims, we should see a negative slope to its line in Figure 1.

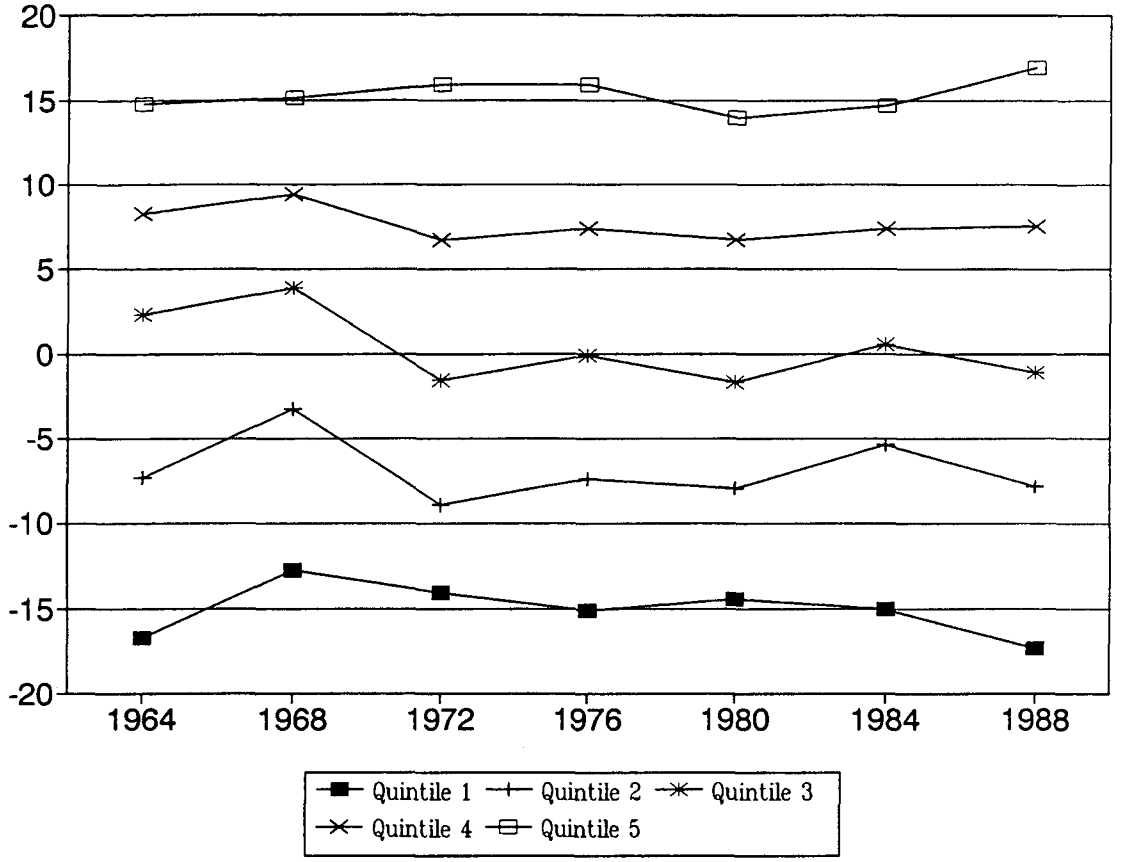
This is not the case for lower-income groups. Between 1964 and 1988, there is only a marginal change in the difference between turnout rates for the two lowest-income groups and the overall turnout rate—from 16.8% and 7.4%, respectively, in 1964, to 17.4% and 7.9%, respectively, in 1988. The greatest decline in turnout relative to the overall rate between 1964 and 1988 actually occurs for the middle-income quintile, with a change from +2.3% to −1.2%, while the only increase in turnout occurs in the highest income group (from 14.7% to 16.8%).

Each of these differences, however, is exaggerated by relatively large changes between 1984 and 1988. If the turnout differences were compared for 1964 and 1984, we could report that relative turnout of the two lowest-income groups had increased, the middle-income group had declined slightly, and the relative turnout of the upper-income groups had basically remained stable.

If we restrict the analysis further to the 1964–80 period used by Burnham (1987a), we would, again, reject the selective demobilization claim. Between 1964 and 1980, the relative turnout of the lowest-income group actually increased slightly—the only group to do so. Slight declines in the turnout difference for the upper two income groups, as well as the next-to-lowest-income groups, occur, while the middle-income group experiences the largest relative decline in turnout across the 1964–80 period. Thus,

FIGURE 1

Difference between Turnout for Each Income Quintile and the Overall Turnout Rate



Source: Computed from the Current Population Surveys, 1964–88.

restricting the time period to that used by Burnham does not change our initial conclusion that socioeconomic class bias in the electorate has not increased since 1964.

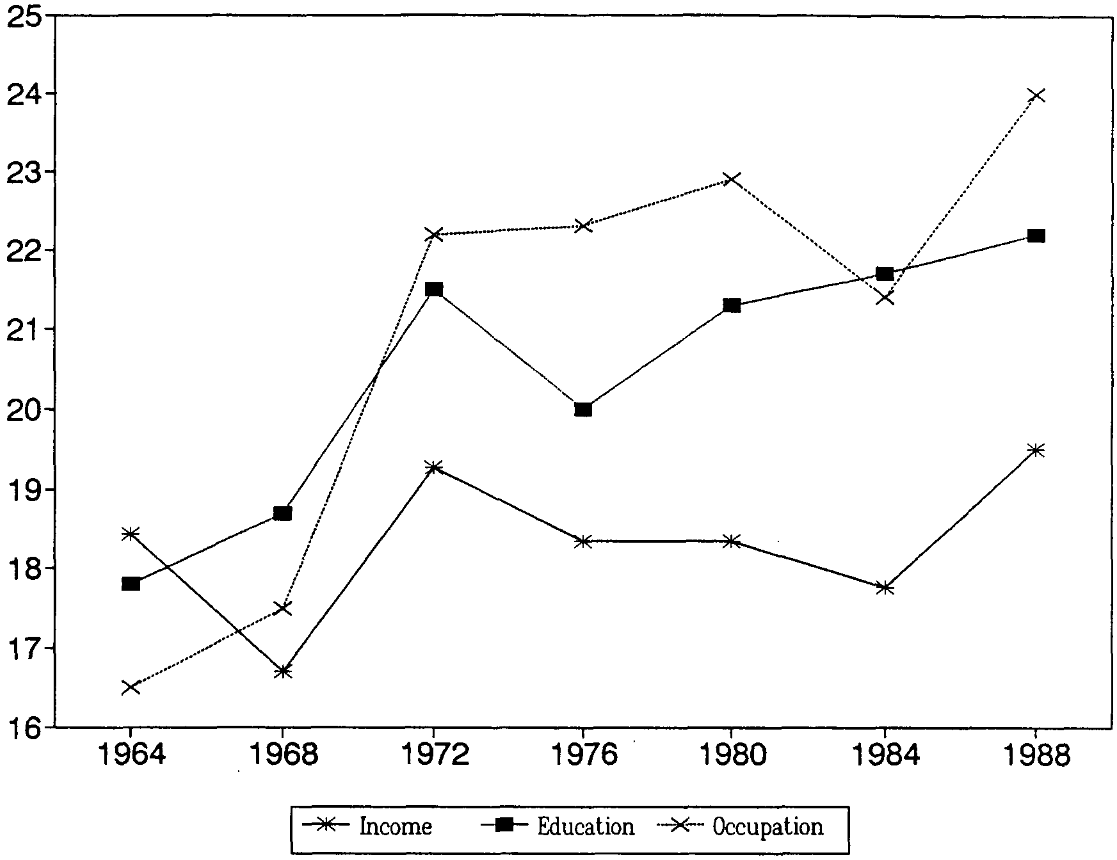
A more precise measure of the relative representation of low-income versus high-income individuals in the electorate is derived from the CPS data using a modification of the Gini coefficient. Gini coefficients are usually used as measures of income inequality of societies (e.g., the amount of wealth held by the richer, compared to the amount held by the poorer, members of society). We have modified the calculation of it slightly to make it a measure of electoral representation for different income groups. Though the Gini coefficient is not typically used in analyses of turnout, it offers two advantages. First, it is a calculation that makes use of all the voters, rather than merely the extremes of the income range used when turnout difference scores are calculated based on the turnout of the highest- versus lowest-income groups. Second, it gives a bounded measure that can be compared across years. These advantages are especially valuable to resolve the class bias dispute, given that findings based on simple comparisons of the lowest- to the highest-status groups may depend upon the size of the groups chosen.

Each year, the CPS categorizes respondents in one of 14 income groups. We computed 14 pairs of points for each year—the percentage of persons at or below each income level paired with the percentage of voters at or below that income level. The pairs were ordered by ascending income level, then plotted on the x- and y-axes. The Gini coefficient is defined as twice the area between this curve and a 45-degree line. If equal percentages of persons voted regardless of their income level, then the pairs should trace a 45-degree line; and the Gini coefficient would be zero. Alternatively, if the only persons who voted were those persons in the highest-income group, then all points except the final one would lie on the x-axis, and the Gini coefficient would be one. Thus, the Gini coefficient is bounded by zero and one, with zero representing perfect equality of representation by income groups and one representing a situation where all of the votes are concentrated in the hands of the top-income group: the higher the Gini coefficient, the more unrepresentative the voting population is.[9]

The Gini coefficients, reported in Table 1, follow a pattern that is the inverse of overall levels of turnout; that is, when turnout decreases, the Gini coefficient increases, and vice versa. During the 1964–88 period,

FIGURE 2

Trends in Class Bias, Using Various Measures of Socioeconomic Status



Note: Income bias is the difference in turnout rates between the top and bottom halves of the income distribution. Education bias is the difference in turnout rates between high school graduates and non–high school graduates. Occupation bias is the difference in turnout rates between white-collar and blue-collar workers.
Source: Computed from the Current Population Surveys, 1964–88.

there is again no secular trend of increasing unrepresentativeness of the electorate based on income. Were the lower-income groups voting less or the higher income groups voting more, we would see a consistent increase in the Gini coefficients over time. Instead, the Gini coefficient is almost unchanged from 1976 to 1980 and drops from 1980 to 1984. It is only in the 1988 election that the Gini coefficient shows a sizable increase.[10]

SELECTIVE DEMOBILIZATION AND ALTERNATIVE MEASURES OF SOCIOECONOMIC CLASS

So far, we have determined that with income as a measure of socioeconomic class, there has been virtually no increase in class bias between 1964 and 1984 and that 1988 stands out in contrast to that stability. To the extent that this finding is solely a function of using income, rather than occupation, as a measure of socioeconomic status, we have not resolved the class bias controversy but, instead, merely added to it. We now explicitly consider Burnham's (1987a) claim regarding selective demobilization by comparing estimates of class bias using occupation groups, education, and income as alternative measures of socioeconomic status. For ease of comparison, we have plotted turnout differences between upper- and lower-status groups using each of the three measures. Figure 2 plots these differences for (1) the top 50%, versus the bottom 50%, of the income distribution; (2) white-collar workers versus blue-collar workers; and (3) high school graduates versus individuals with fewer than 12 years of school.[11]

So far as education, income, and occupation reliably measure the same underlying trait (socioeconomic status), each indicator of socioeconomic class should generally follow the others across time. According to Figure 2, the level of socioeconomic bias associated with occupation increased significantly between 1964 and 1988. The lower occupation group (manual labor) has a sharply reduced voting rate vis-à-vis the higher occupation group (white-collar workers) in 1988 compared to 1964; the gap is 24% in 1988 compared to 16.5% in 1964. Consistent with

TABLE 2

Turnout by Demographic Characteristics (1960–88, National Election Studies)

DEMOGRAPHIC GROUP	1960	1964	1968	1972	1976	1980	1984	1988
Total	82.2	77.7	75.8	72.8	71.6	71.4	73.6	69.7
Income								
1 (Low)	65.2	63.8	60.5	60.1	54.3	56.4	52.7	47.4
2	72.4	72.9	66.1	62.9	65.4	68.0	69.3	58.9
3	85.0	79.0	79.3	70.5	70.9	72.1	74.5	71.0
4	89.9	85.8	86.8	86.2	80.4	81.4	84.4	82.4
5 (High)	96.2	87.8	92.5	90.4	91.4	86.7	91.3	95.6
Occupation								
Blue-collar	81.3	74.4	69.1	68.3	65.1	63.9	64.4	54.7
White-collar	91.9	85.2	86.4	82.1	82.2	81.4	83.3	81.2
Education								
No high school	70.5	67.8	60.4	58.0	59.8	58.6	58.0	50.0
Some high school	75.2	71.3	68.6	60.8	57.2	54.8	56.1	49.8
High school grad	89.2	80.3	82.7	74.7	67.0	61.4	65.9	62.3
High school plus training	93.6	85.1	85.8	76.5	73.9	77.8	73.8	—
Some college	90.5	88.6	79.1	83.9	83.3	75.8	81.5	78.4
Post-college	96.3	87.7	89.4	89.9	87.1	90.7	90.6	92.4

Note: Table entries are the reported turnout rates for each demographic category.

Burnham (1987a), this gap is also evidenced when comparing 1980 to 1964.

Figure 2 also shows that the socioeconomic bias associated with income—the difference in turnout between the top 50% and bottom 50% of the income distribution—rose from 1968 to 1972. However, the increase is less than half the increase shown by the occupation gap. Furthermore, the income gap in 1968 was exceptionally low; the 1972 income gap is actually less than 1% above the 1964 income gap. The socioeconomic bias associated with education—the gap between turnout rates of high school graduates and nongraduates—also increased significantly less from 1968 to 1972 than did the occupation gap, according to Figure 2. Thus, the substantive conclusions drawn regarding selective demobilization are indeed sensitive to the choice of measure used. Claims regarding selective demobilization cannot be sustained when using income as a measure of socioeconomic status.

Several points cause us to question the validity of occupation as a measure of socioeconomic status. First, the majority of the drop in relative voting rates among the occupation groups occurred in one time period: from 1968 to 1972 the voting gap between the two groups increased from 17.2% to 22.2%. This suggests that rather than being a secular trend over the time period identified by Burnham (1987a), the turnout decline for manual labor actually occurred between the 1968 and 1972 elections.[12]

Second, the education gap, as well as the income gap, decreased in 1976, while the occupation gap continued to rise. If the variables plotted in Figure 2 measure the same phenomena—turnout differences as a function of socioeconomic status—then they should move in the same direction (and, ideally, with the same magnitude) from year to year.

Third, were occupation a reliable measure of socioeconomic class during this period, then we should see similar patterns of turnout for occupational groups using the NES. Instead of replicating the CPS findings, the NES shows quite different patterns of turnout for blue- and white-collar workers over time (see Table 2). For instance, between 1968 and 1972, when Burnham (1987a) reports a significant change in relative turnout levels, the NES data show that turnout for blue-collar workers is fairly stable (69.1% in 1968, 68.3% in 1972), while turnout for white-collar workers drops from 86.4% in 1968 to 82.1% in 1972. Thus, the turnout differences between white-collar and blue-collar workers for 1968 and 1972 are 17.4% and 13.8%, respectively. This suggests that across these two elections, with occupation measures from the NES, socioeconomic class bias actually *decreases*.

Having presented evidence suggesting that occupation is less reliable an indicator than education or income, we now offer an alternative explanation for the drop in relative turnout between 1968 and 1972 among the lowest occupation categories observed by Burnham (1987a). Since these two elections spanned a decennial census, the changes the Census Bureau made in their occupation codes could account for the discrepancy between the pre- and post-1970 turnout rates. In the absence of supporting evidence for the sudden drop among the low occupation category and in the face of conflicting turnout patterns using the NES data, the coding change hypothesis must be considered a plausible explanation for the figures.

TABLE 3

Misreporting by Occupation and by Income (1964, 1976–88, National Election Studies)

DEMOGRAPHIC GROUP	1964	1968	1972	1976	1980	1984	1988
Occupation							
Blue-collar	40.3	—	—	31.8	21.2	23.7	16.1
White-collar	53.0	—	—	44.5	45.0	37.5	38.6
Income							
Low	37.4	—	—	31.3	26.1	21.6	16.8
High	45.2	—	—	34.7	33.7	33.6	33.2

Note: Table entries are the percentage of *nonvoting* respondents in each category who claim to have voted.

This reflects the basic problem with the use of the occupation variable: it is fundamentally a categorical, rather than ordinal, measure.

REPORTED VERSUS VALIDATED TURNOUT

To verify that these findings do not result from changes in the tendency of high- or low-income—or blue- or white-collar—individuals to misreport voting, we also examined turnout using the NES validated vote. If low-income individuals or blue-collar workers are now voting in lower numbers but misreporting their behavior more frequently, then we would fail to find an increase in socioeconomic class bias using the reported CPS vote. If the high relative misreporting rate for blacks observed by Abramson and his coauthors is a recent phenomena and extends more broadly to lower socioeconomic groups, then our finding of no increase in class bias could be erroneous. To test our earlier results we calculated the probability of individuals' misreporting for each income and occupation group conditioned on their not having voted. (Almost no one votes and reports that they did not.)

These results are reported in Table 3, which shows that in fact, over time, there have been rather large changes in misreporting probabilities. While both occupational groups show less of a tendency to misreport in 1988 than in 1964, the decline in misreporting is much larger for the blue-collar workers. Misreporting by white-collar individuals decreased from 53.0% in 1964 to 38.6% in 1988; among blue-collar individuals the decrease was even larger: 40.3% in 1964 compared to 16.1% in 1988. Thus, the difference in misreporting *increases* from 12.8% in 1964 to 22.5% in 1988, with higher-status individuals being relatively more likely than lower-status individuals to misreport in 1988 than in 1964. Using the reported vote, differences in turnout between classes would be more exaggerated in 1988 than in 1964.

We find a similar result when comparing misreporting by income. Misreporting for the bottom half of the income distribution decreases from 37.4% in 1964 to 16.8% in 1988, while for the top half of the income distribution it decreases from 45.2% in 1964 to 33.2% in 1988. Hence, the difference in misreporting rates again *increases* from 7.8% in 1964 to 16.4% in 1988. Analyzing reported vote over time would tend to exaggerate increases in socioeconomic class bias since the higher-class groups' turnout would be falsely overreported. Hence, using the reported vote to claim stability in the socioeconomic voting gap is a conservative test and does not call into question the validity of our finding that socioeconomic status bias has remained stable.[13]

MULTIVARIATE MODEL

We now adopt a multivariate model that allows us to assess the marginal impact of each demographic characteristic on voter turnout. As a further test of our claim that there has been little or no change in socioeconomic class bias, we use this model to test the stability of the marginal effect of income (other demographic traits held constant) across elections. In this model, we include the basic individual demographic characteristics found to be correlates of voting: education, income, age, sex, marital status, and race. We also include a dummy variable to denote the South. Since the effect of age on turnout appears to decrease at some point in the individual's life cycle, we also include an age-squared term in the model, with the expectation that its estimated coefficient would be negative.

The probit estimates of this model for the reported vote are presented in Table 4. The stability of the coefficients of income and education (they are bounded between .115 and .139 and between .266 and .283, respectively, for each year) is further evidence that there has not been a major shift in the impact of socioeconomic status on voting. In other words, had high-status individuals become more likely—and low-status individuals less likely—to vote, then the effect of education and income as independent predictors of turnout should have increased over time. That the estimate for income decreases slightly (.126 vs. .115), and the coefficient

TABLE 4

Probit Estimates of the Demographic Model of Turnout (1972–88, Current Population Surveys)

VARIABLE	1972	1976	1980	1984	1988
Constant	−2.612	−2.927	−2.943	−2.884	−3.061
	(.038)	(.040)	(.033)	(.036)	(.038)
South	−.235	−.107	−.103	−.120	−.121
	(.011)	(.011)	(.010)	(.011)	(.010)
Education	.266	.275	.281	.280	.283
	(.0037)	(.0039)	(.0033)	(.0036)	(.0038)
Income quintile	.126	.129	.115	.125	.139
	(.0039)	(.0042)	(.0033)	(.0036)	(.0039)
Age	.052	.056	.054	.052	.049
	(.0015)	(.0016)	(.0014)	(.0015)	(.0015)
Age squared	−.00034	−.00036	−.00033	−.00030	−.00026
	(.000017)	(.000017)	(.000014)	(.000015)	(.000015)
Male	.026	−.018	−.048	−.083	−.055
	(.0095)	(.0100)	(.0084)	(.0089)	(.0092)
Black	.098	.061	.040	.235	.147
	(.016)	(.017)	(.014)	(.015)	(.016)
Marital status	.105	.118	.161	.122	.153
	(.011)	(.012)	(.010)	(.010)	(.010)
Number of cases	83,360	75,644	107,200	95,380	89,423
% correctly predicted	70.88	70.12	70.63	71.01	70.58
% voted	65.44	65.23	63.85	64.90	62.64
Log likelihood	−47,259	−43,327	−60,685	−53,616	−50,593

Note: All coefficients are significant at $p < .01$. The dependent variable is the respondent's reported turnout for each presidential election. Table entries are probit coefficients, with standard errors in parentheses.

for education remains essentially unchanged (.266 vs. .281) from 1972 to 1980—the endpoint for Burnham's (1987a) calculation—suggests that class bias remained essentially the same over that period.

To interpret the probit coefficients, predicted probabilities based on changes in the independent variables should be examined. The predicted probability of voting is equal to $\Phi(X_i\hat{B})$, where $\Phi(X_i\hat{B})$ is the cumulative normal evaluated at the sum $X_i\hat{B}$ (Johnston 1984). Therefore, the predicted probability will depend upon the value of $X_i\hat{B}$, or of the values of all the independent variables. The effect of a change in an independent variable can be calculated by computing two predicted probabilities: an initial probability based on specific values of the independent variables, and an adjusted probability based on a change in the independent variable of interest. For instance, the effect of moving from the middle-income quintile to the next highest quintile for a 30-year-old, single, white northern male with high school education in 1972 would have been to increase the likelihood of voting by 4.8% (from 59.9% to 64.7%).[14] In 1988, the effect would have been to increase the likelihood of voting by 5.5% (from 42.6% to 48.1%), meaning that the effect of income has increased for such an individual by only .7% over the five elections. Thus, even when considering 1988, there is not evidence of a large increase in class bias.

Additional evidence that class bias has not increased significantly is provided by estimating the same multivariate model using the NES validated vote. As reported in Table 5, the coefficient of the income variable is quite unstable over time. In particular, the increasingly large estimates of the impact of income on turnout between 1980 and 1988 derived from the census data are not replicated using the validated vote. Instead, the income coefficient is at its lowest in 1980 (.056), increases to .190 in 1984, and then drops to .104 in 1988. Thus, even were we to extend the time period under consideration to assess Burnham's argument, we find no support for dramatic increases in class bias.

Tables 4 and 5 indicate that the effects of two socioeconomic characteristics other than income, occupation, and education *have* changed as predictors of turnout between 1964 and 1988. The estimates for sex show a noticeable trend toward increased voting by women relative to men. The coefficient of race is much less stable than the other estimated demographic coefficients. According to the CPS data, the effect of being black on turnout is positive, though it decreases in magnitude throughout the 1970s, then increases in the 1980s. The validated NES data suggest that the effect of being black decreases the likelihood of voting, with the magnitude of the impact increasing throughout the 1980s. Thus, while there may be changes in the composition of the electorate over time, they are associated with less class-based measures than income, occupation, or education.

TABLE 5

Demographic Model of Validated Turnout (1964, 1976–88, National Election Studies)

VARIABLE	1964	1976	1980	1984	1988
Constant	−1.793**	−2.432**	−2.498**	−2.045**	2.570**
	(.354)	(.226)	(.289)	(.242)	(.261)
South	−.768**	−.329**	−.204*	−.274**	−.387**
	(.095)	(.068)	(.082)	(.070)	(.072)
Education	.121**	.154**	.240**	.162**	.214**
	(.027)	(.021)	(.027)	(.024)	(.023)
Income quintile	.142**	.165**	.056	.190**	.104**
	(.041)	(.034)	(.041)	(.034)	(.036)
Age	.055**	.059**	.054**	.039**	.058**
	(.015)	(.010)	(.013)	(.011)	(.011)
Age squared	−.0003*	−.0004**	−.0003**	−.0001	−.0003**
	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)
Male	.030	.017	.028	−.171**	−.050
	(.079)	(.060)	(.076)	(.063)	(.068)
Black	.054	.090	−.059	−.069	−.164
	(.141)	(.102)	(.122)	(.101)	(.099)
Marital status	−.014	.189**	.282**	.193**	.262**
	(.105)	(.068)	(.085)	(.069)	(.074)
Number of cases	1,218	1,708	1,301	1,902	1,714
% correctly predicted	70.9	69.2	67.9	68.0	70.4
% voted	65.8	64.5	58.9	62.8	57.7
Log likelihood	−697.9	−1,252.5	−780.9	−1,108.1	−991.7

Note: The dependent variable is the validated measure of the respondent's turnout in each presidential election. Table entries are probit coefficients, with standard errors in parentheses.

$^{*}p < .05$

$^{**}p < .01$

CONCLUSION

The question whether class bias in the electorate has changed over time seems straightforward. However, the debate it has engendered suggests otherwise. We have attacked this question in as direct a way as possible. By tracing the demographic characteristics of all voters (as opposed to particular subsets of voters) over seven consecutive presidential elections and using samples large enough to reduce random error, we found that class bias has not increased since 1964. This conclusion is buttressed by a comparison of different distributions of income over time using Gini coefficients, as well as by multivariate analyses.

Any conclusion regarding class bias in the electorate is dependent upon the choice of measure of socioeconomic status. We argue that from a theoretical perspective, income is the appropriate measure to use, since government policy is more likely to distinguish among individuals based on income, rather than any other single measure of socioeconomic status. And we point out that there are measurement problems with alternative indicators, such as occupation.

We have painted a picture of turnout that is remarkably stable over seven different elections spanning 24 years. Despite different social and political contexts and different candidate choices, the class differences between voters and nonvoters in presidential elections remain the same. These elections include races between incumbents and challengers, as well as two open seat races. During these elections, the electorate faced a broad array of issues, ranging from Vietnam to double-digit inflation to $200 billion budget deficits. The one deviant election we observed was 1988, where socioeconomic class bias in turnout did seem to increase. The class consequences of Reagan administration policies beginning in 1981 may have influenced the composition of the electorate in 1988 (see Phillips 1990). Alternatively, campaign-specific effects such as the content of television advertising or the targeting strategy of either campaign could account for the distinctiveness of the 1988 electorate. Whether these differences persist in the 1992 election is obviously an unanswered question.

One can interpret these findings either optimistically or pessimistically. From a normative perspective, the lack of an increase in class bias in the electorate is good news. However, from the representational perspective of desiring an electorate able to adapt to changing circumstances and choices, the stability is not necessarily good news.

TABLE A-1

Size of Income Quintiles: Percentage of Eligible Voters in Each Group

INCOME	1964	1968	1972	1976	1980	1984	1988
Current Population Surveys							
1 (Low)	19	27	18	17	20	22	17
2	20	24	22	23	15	21	22
3	28	19	15	24	25	16	19
4	16	20	25	15	14	24	25
5 (High)	18	10	19	21	26	17	18
National Election Studies							
1 (Low)	19.1	15.7	19.2	14.1	17.4	15.2	19.1
2	18.4	21.0	11.8	19.2	15.6	17.7	13.2
3	26.0	31.5	37.2	31.0	36.2	35.5	35.4
4	29.5	26.7	26.8	30.5	25.4	26.0	29.7
5 (High)	7.0	5.2	5.1	5.2	5.4	5.6	2.5

APPENDIX

The exact size of each income quintile is shown in Table A-1.

Notes

This is one of several joint papers on turnout in American national elections; the ordering of the authors' names reflects alphabetic convention. We appreciate constructive comments on earlier versions of this paper from Frank Baumgartner, Wendy Hansen, Kim Hill, Patricia Hurley, Rod Kiewiet, Rebecca Morton, Doug Rivers, and Larry Rothenberg. We would like to acknowledge the research assistance of Sam Hudson and typesetting assistance of Marcia Bastian. Research support from the Texas A&M University College of Liberal Arts and Supercomputer Center is also appreciated. The data utilized in this publication were made available in part by the Inter-University Consortium for Political and Social Research. The data for the American National Election Studies, 1964–88, were originally collected by Warren E. Miller and the National Election Studies.

1. See Burnham 1982 and Kleppner 1982 for discussions of historical trends in turnout and representation.

2. For instance, to conclude that coefficients from consecutive elections are even significantly different from each other using an NES data set that typically generates income coefficients with t-statistics of about 4—much less to estimate the magnitude of the difference—would require that one estimated coefficient be over twice the size of the other.

3. We have corrected Census Bureau–reported figures for 1964 and 1968 to be consistent with our method of computing turnout.

4. For a discussion of the properties of the Census Bureau estimates, see Jennings 1990.

5. No validation study was conducted in 1968; and though a validation variable is available for 1972, the CPS discourages its use because the validation procedure for the 1972 vote was conducted in 1976 and produced poor estimates of validated turnout (Traugott 1989).

6. Bennett 1991 finds an increase in class bias among older whites, as well, but notes that it is not nearly as large: the gap in turnout between the two groups increases only 4%.

7. Directly comparable census figures are not available. However, according to the census data, 30% of individuals over age 20 had some college experience in 1964, and 38.8% of individuals over age 20 had such experience in 1988.

8. In Burnham's many writings on the subject of social class and American political behavior, he has used a variety of measures of social class, including income, education, housing values, and homeownership (1978, 1987b). But on the narrow question of changes in class bias after 1960, he consistently uses Census Bureau occupation data as a measure of social class.

9. The Gini coefficients essentially incorporate the same information that would be contained in a regression of voting rates against income: the coefficient of such a regression would represent the best way to fit all the voters to the data. Computing regression coefficients for the five years using the voting rate of each income quintile as the independent variable yielded coefficients consistent with the estimated Gini coefficients: the smallest coefficient was for 1984, the largest for 1988.

10. Since the raw data was unavailable for 1964 and 1968, we could only compute Gini coefficients for those years using the six income categories contained in the Census reports. These coefficients (.0856 and .0770) were lower than the Gini coefficients we computed for subsequent years. However, their reliability is extremely suspect, because Gini coefficients calculated with fewer income groups would necessarily be smaller than those calculated with income groups representing a finer breakdown of the income distribution.

11. These income gaps are not exactly the top 50%, versus the bottom 50%, of the distribution, because income is a categorical, rather than continuous, variable in the survey. However, in each year, the difference between the size of the top and bottom group is no more than 8%. The size of the blue-collar group ranges from 37.1% to 30% of the workforce, the size of the white-collar group ranges from 45.4% to 56.7%. In 1964, only 52.4% of respondents were high school graduates; by 1988 they had reached 77.4%.

12. We also examined the sensitivity of the occupation variable to disaggregation by sex, since Burnham only considered the turnout of men. The 1968–72 jump was identical for men and women. After 1972, the gap for women decreased; but the overall trend remained the same as the trend for men only.

13. See Filer, Kenny, and Morton 1991 for estimates of demographic voting based on county turnout figures (hence uncontaminated by misreporting problems).

14. In the CPS data, income quintiles are coded from 0 to 4 (4 being the highest), education is coded from 1 to 8 (8 being highest).

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TOWARD A THEORY OF THE POLITICAL ENTREPRENEUR: EVIDENCE FROM LOCAL GOVERNMENT

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Political scientists have been increasingly interested in entrepreneurs—individuals who change the direction and flow of politics. In this research note, we synthesize aspects of an economic approach to entrepreneurship with concepts used in political science. We then tie these theoretical observations to the emergence of entrepreneurs in local governments and test components of our theory using observations from a large set of suburban municipal governments. Empirically, we identify several conditions that affect the probability that an entrepreneur will emerge in a local government, especially slack budgetary resources that the political entrepreneur can reallocate. We also find that the probability with which an entrepreneur is found in local government is a function of the difficulty of overcoming collective action problems in a community.

Political scientists have been increasingly interested in entrepreneurs—individuals who change the direction and flow of politics. Existing studies of political entrepreneurship rely heavily on case study material. Most common are biographical studies of entrepreneurial leaders, which focus on individuals whose actions produced policy changes that were innovative or unexpected (see, e.g., Doig and Hargrove 1987; Kirchheimer 1989; Lewis 1980; Riker 1986; Weissart 1991). Such studies tend to focus on high-level bureaucrats in governmental or quasi-governmental agencies (Robert Moses, David Lilienthal, Admiral Hyman Rickover) or on dynamic politicians (Abraham Lincoln, Lyndon Johnson). Another case study approach to entrepreneurship is through the use of issue histories, which analyze the role of policy entrepreneurs in the development and implementation of new public programs (Beam 1989; Kingdon 1984; Milward and Laird 1990).

More recently, formal models of leadership have been presented (e.g., Bianco and Bates 1990; Calvert 1987). These formal theories focus on agenda setting, strategic manipulation, and the incentives of leadership. Riker's (1986) notion of "heresthetics" has been particularly important in stimulating this more systematic approach to the study of leadership by political scientists. More theoretic than most biographies or issue histories, yet not fully "formal," Riker's work shows how a political entrepreneur can advantageously transform existing political coalitions, especially by adding a new dimension to political debates. Importantly, this line of research transforms the notion of entrepreneurs from the study of heroic figures to the study of a larger class of individuals who help propel political and policy changes.

Most case studies lack generalizability, and formal models are necessarily abstract and fairly simple compared to real political situations. To understand political entrepreneurship we need to further develop elements of a theory of entrepreneurship and empirically test these theoretical expectations. We shall synthesize aspects of an economic approach to entrepreneurship with concepts used in political science and apply the results to local government.

We argue that there is a population of potential political entrepreneurs distributed across local governments. The size of the local population with entrepreneurial skills and ambitions is a function of the characteristics of the community, such as its income and education level (Ricketts 1987; Ronen 1983, intro.).[1] While we cannot observe the distribution of potential entrepreneurs, our research does identify communities in which entrepreneurs are found. Our empirical research goal is to identify the conditions that affect the probability with which an entrepreneur emerges in any given community.

Potential entrepreneurs have energies and talents that they could invest in alternate spheres of activity. The rate at which they are attracted to the local *political* environment (as compared to some other domain) is a function of the costs they face in entering the political arena and the benefits they garner if they succeed as political entrepreneurs (Ricketts 1987). The level of these costs and benefits are embodied in measurable facets of local government. For example, we shall argue that costs are a function of the collective action problem entrepreneurs face and the ease with which these problems can be solved. Similarly, the benefits (or "profits") entrepreneurs reap are a function, among other things, of the budgetary slack of their local community, which affects the entrepreneur's ability to reallocate resources to achieve the policy goals held by the entrepreneur.

These factors enter the decision calculus of the *individual* entrepreneur. But the decisions of these individuals have wider impacts. On a more *systemic* level, entrepreneurs can affect the political equilibrium in metropolitan areas. For example, within a community an entrepreneur can transform the nature of political debates (Riker 1986; see also Schneider and Teske n.d.). Across communities, the entrepreneur can change the service/tax mix found in communities facing each other in the local market for public goods (see Tiebout 1956; Schneider 1989).

We shall turn first to the conceptually more difficult issue of entrepreneurs and equilibrium, then to an investigation of entrepreneurial profits and the difficulties entrepreneurs face in organizing a collective base to launch entrepreneurial policies. Finally, using observations from a large set of suburban municipal governments, we shall empirically address the question, Under what local conditions are political entrepreneurs more or less likely to be found?

HOW ENTREPRENEURS AFFECT MARKETS

To understand how entrepreneurs affect markets and politics, we begin by considering private sector entrepreneurs, comparing the role of the entrepreneur in the private market to the role of political entrepreneurs. We are directly concerned with entrepreneurs in what we call the local market for public goods, in which local services are delivered by multiple municipal governments in packages that are paid for using a revenue system in which local property taxes are central.[2]

While we develop a variety of theoretical issues that underlie the study of political entrepreneurship, two are central to the empirical analysis we present later. First, we are concerned with the concept of "opportunity": What conditions lead a potential entrepreneur to become active in the local political arena? Most economic theories of the entrepreneur argue that the pursuit of profits drives the entrepreneurial process. Hence, we are interested in identifying the conditions that affect potential profits in the local political arena and thus affect the likelihood that a political entrepreneur will be found. A second issue centers on the organizational milieu of communities that makes entrepreneurship more or less likely. We tie these conditions to the ability of the entrepreneur to solve problems of collective action, problems that are far more critical to the political entrepreneur than to most economic entrepreneurs.

Entrepreneurial Profits and Market Equilibrium

Perhaps the most common focal point in the recent economic literature on entrepreneurship is the emphasis on the "discovery" of market opportunities by "alert" individuals (see Kirzner 1985). From this perspective, entrepreneurs engage in the act of "creative discovery"—they try to take advantage of newly *discovered* or newly *created* possibilities in order to earn entrepreneurial profits (Casson 1982; Ricketts 1987).

In their drive for profits, private entrepreneurs discover and exploit new opportunities through arbitrage, speculation, or innovation. Entrepreneurs are driven by personal motives and the maximization of profits, but their activities affect the overall market in which they operate. Disagreement over the degree of independence of entrepreneurs distinguishes the Austrian school's approach to the study of entrepreneurs from that of the neoclassical "establishment." Moreover, economists do not agree on how entrepreneurs affect market equilibrium.

To summarize a large, sometimes difficult, and often contentious literature, entrepreneurs are not central to much of neoclassical economic analysis. Hayek believed that neoclassical economics neglected entrepreneurs because its core concept of market equilibrium assumes full information, leaving the entrepreneur little scope for innovative activity (Hayek 1949; Kirzner 1985). According to Kirzner, the "neoclassical approach has . . . squeezed the real-world entrepreneur back into the neoclassical full-equilibrium box" (1985, 8–9). In the neoclassical approach, entrepreneurs are "called forth" by conditions in the market in a systematic, if not necessarily predictable, process. In contrast, in the Austrian tradition, entrepreneurs have greater independence of action, and market outcomes are less predetermined. However, in both schools of thought, the relationship between entrepreneurs and equilibrium is central.

On one hand, entrepreneurs can be viewed as seizing opportunities in the market to move it toward equilibrium. While Kirzner's entrepreneur is defined by an "alertness to disequilibrium" and independently injects new elements into markets, ultimately, for Kirzner,"—the changes the entrepreneur initiates are always toward the hypothetical state of equilibrium" (1973, 73). In contrast stands Schumpeter's idea of creative destruction (1942; see also Shackle 1972). For Schumpeter, entrepreneurial activity is most likely to emerge when a market is approaching equilibrium. In this evolving situation, the successful entrepreneur engages in a series of innovations that move the market first toward disequilibrium and then, ultimately, to a new market arrangement that is more profitable for the entrepreneur.

Political scientists studying entrepreneurs are struggling with similar issues. We have very little information about variation in the supply of political entrepreneurs, that is, how and why political entrepreneurs are called forth. Kingdon's work (1984) may be the best current description of how entrepreneurs market their policy ideas to national decision makers. Kingdon describes policy entrepreneurs as constantly shopping around for windows of opportunities through which to shove their preferred policy ideas (also see Walker 1981). In a similar vein, Elder and Cobb (1983) focus on the importance of policy entrepreneurs in agenda setting.[3] In an extension of this approach, Baumgartner and Jones (1991) argue that the outcome of political debates can vary with the arena or "venue" in which debates occur. Because the federal system of the United States presents so many alternate venues, policy entrepreneurs can maximize their likelihood of success by carefully shopping around for the most favorable available venue. While these works focus on the role of entrepreneurs, they do not systematically identify the conditions under which entrepreneurs emerge.

Political scientists also need to explore further the relationship between entrepreneurs, innovation, and equilibrium. Since the concept of equilibrium is at the core of so much economic analysis, the extent to which entrepreneurs move private markets toward equilibrium or disequilibrium is theoretically critical for their enterprise. But the concept of equilibrium is also important in the study of entrepreneurs in *political* marketplaces.

Again, Riker's position is critical. Riker (1982) argues that political situations are normally characterized by an underlying disequilibrium of tastes. His contention is supported by a number of experimental and formal analyses that have severely restricted the conditions under which equilibrium can be expected. The fact that stable "equilibrium" outcomes are empirically evident despite the underlying dynamics of instability presents a conundrum for political scientists. Shepsle (1979) was among the first to show how institutions can induce stable outcomes despite the forces pushing toward disequilibrium. More recently, using field studies exploring the use of common property resources, Ostrom (1990) has shown how a wide variety of societal arrangements can overcome strong inherent tendencies toward disequilibrium driven by conflicts in tastes and strategies.

Thus, the idea of institutionally induced equilibrium provides a line of argument that addresses one part of the equilibrium/disequilibrium conundrum. But this makes Riker's argument linking politicians and equilibrium an even more central issue that must be addressed in any theory of entrepreneurship. For Riker, while equilibrium may appear in political arrangements at any given point in time, such an equilibrium is inherently unstable. This presents openings for Riker's "heresthetician"—an entrepreneurial political leader who "probes until he finds some new alternative, some new dimension that strikes a spark in the preferences of others" (Riker 1986, 64). Riker argues that through agenda control, strategic behavior, and (most importantly) the introduction of new policy dimensions to political debate, the heresthetician can break up institutionally induced and maintained equilibria to create new and more profitable political outcomes. Riker's heresthetician thus resembles Schumpeter's entrepreneur: both engage in creative destruction, tearing apart existing political–economic arrangements in order to create new ones.

Like Schumpeter's entrepreneur, Riker's heresthetician stands to profit from the successful introduction of new ideas or policies into a market that appears stable to others. After the disruption caused by the innovation, a new stability may emerge. But Riker believes that lurking behind this new stability is still the fundamental disequilibrium of taste that can then be exploited by the next generation of entrepreneurial politicians. Similarly, for Schumpeter, as the market absorbs the last round of innovations to approach equilibrium, the potential for entrepreneurial profits increases, giving rise to yet a new generation of entrepreneurs. Empirically, the outcome of innovations pushed by Schumpeter's entrepreneur or by Riker's heresthetician may thus resemble a *punctuated equilibrium*—a process in which institutions are characterized by long periods of stability punctuated by short bursts of rapid change (Krasner 1984). In the realm of politics it is important to remember that during these periodic disruptions, not only are the present politically determined allocations of resources questioned, but the fundamental rules of the game may also be challenged.

In the arena of local governments with which we are ultimately concerned, governments are in competition with one another and use their taxes and services to lure desirable households and business firms (Peterson 1981; see Schneider 1989). In the resulting competitive market, some forms of local political entrepreneurship may temporarily create disequilibrium, upsetting existing local public service and tax arrangements and altering the relationship between communities in a region. These innovations are often then absorbed and institutionalized into new political arrangements and a new balance between communities emerges. As we shall argue, the tax/expenditure limitations movement of the 1970s (e.g., Clark and Ferguson 1983), the concern for economic development policies of the 1970s and 1980s (Eisinger 1988), and the recent no-growth movement (see Logan and Rabrenovic 1990; Schneider 1992) may all be examples of entrepreneurship in local government that display some of the dynamics of punctuated equilibria.

ELEMENTS OF ENTREPRENEURSHIP: PROFITS AND COLLECTIVE ACTION

Before turning specifically to entrepreneurship in local government, we consider in more detail the concept of entrepreneurial profits. In particular, we explore two specific aspects of entrepreneurship that affect profits: private information and entry barriers.

Entrepreneurial Profits

Virtually every theory of the entrepreneur in the private sector emphasizes the importance of the profit motive. In such theories, the entrepreneur enters into market-making activities with a belief that the existing allocation of resources is somehow wrong and that profits can be made by reallocating them in a correct manner. However, a theory of political entrepreneurship based on the concept of monetary profit will not work. Most political entrepreneurs cannot be residual claimants to profits generated by their ideas (but see Niskanen 1975). This means that one of the most important motivating concepts in economic theories of the entrepreneur cannot be easily employed in the study of political entrepreneurs. Instead, a much more complicated utility function for the political entrepreneur must be developed, including terms related to policy success and status. Formal models of political entrepreneurs pay more explicit attention to this issue than do case-study-oriented analyses. However, these formally derived utility functions are often so complex as to be beyond empirical confirmation. We shall develop two empirical indicators of potential profits for local entrepreneurs; but ultimately, political scientists interested in entrepreneurship must more fully develop the concept of political profits and its indicators.

Private Information. Understanding entrepreneurial profits requires analysis of the ability of the entrepreneur to restrict competition. One way to restrict competition is by maintaining control over information, excluding others from the data necessary to profit in a market.

Thus, one key to entrepreneurial success in the private sector is the protection of private information. Entrepreneurs glean some new piece of information from the environment and then bet on the future based on their insight. However, once this information becomes public, the entrepreneur loses the competitive edge and entrepreneurial profits are bid away. For example, in the process of arbitrage, the entrepreneur sees a discontinuity in the market. Acting on this insight, the entrepreneur enters into a series of profitable transactions. But as the entrepreneur succeeds, the insight becomes more widely known and competitors enter the field, ultimately driving down the entrepreneur's profits. Over time, the entrepreneur's innovation becomes a more or less standardized product, yielding only market rates of return.

Casson argues that to maintain the information advantage, the entrepreneur must be in close contact with *primary* sources of information (1982, 147). The more removed the entrepreneur from such sources, the more distortions enter into the flow of information and the more quickly the entrepreneur's advantage is dissipated. While this observation may have particular relevance for a study of local political entrepreneurs, Lewis's (1980) discussion of Admiral Hyman Rickover presents a clear example from the public sector (also see Bendor, Taylor, and Van Galen 1987 on bureaucratic models of information flow).

Still, there are serious theoretical problems when the concept of private information is carried over to the public sector. What, if any, private information can public sector entrepreneurs monopolize? The very nature of public entrepreneurship often requires that at critical times (especially during elections) the entrepreneur must share information with the public, without being able to protect the rate at which it disseminates to other competing politicians. In order to raise capital, the private sector entrepreneur may have to share his insights with others. But this process of revelation is protected by patent laws, franchising agreements, and so on. In the public sector, there is no similar legal protection on sharing information. And while policy entrepreneurs may emphasize the benefits of their proposals and try to hide the negative aspects (Arnold 1990), the public sector entrepreneur still must disclose more information than the private entrepreneur. In large part, this is because the private sector entrepreneur usually needs to raise capital to operate in the private market—and capital is controlled by a small number of individuals. Most public sector entrepreneurs need votes or support in public opinion to succeed. As Doig and Hargrove (1987) and others note, building and maintaining a political coalition is critical to the success of a political entrepreneur, and revealing information and ideas is central to this coalition process.[4] Indeed, ambitious local entrepreneurs may seek to have their innovations imitated elsewhere, to develop a broader reputation and to increase the rewards of their entrepreneurial activities.

Barriers to Entry. Entrepreneurs will inevitably face barriers embedded in the status quo as they push their innovations. If they are successful, entrepreneurs will themselves seek to establish entry barriers to protect their profits from new entrepreneurs. In the private market, such entry barriers may include maintaining secrecy over technologies and protecting ideas through patents, acquiring monopoly control over critical resources, and creating and maintaining goodwill with customers.

Political science has developed a literature on entry barriers that may be applicable to the study of the entrepreneurial process. While not phrased in terms of entrepreneurship, we know that incumbent politicians use many strategies that restrict competition, limiting the ability of challengers to enter political contests. Most of the tools that incumbents use seek to diminish the number and quality of candidates that might otherwise emerge to challenge the incumbent (e.g., Parker 1991; Tullock 1965). Of particular importance may be the ability of incumbents to advance the interests of their constituents by manipulating bureaucratic services and by controlling the flow of legislation that constituents and contributors need (Fiorina 1977; Parker 1991). The increasing use of personal contact as a form of political advertising also increases the entry barriers challengers face, especially since much of this contacting is subsidized by the government (Fenno 1978; Parker 1986). While the use of the congressional franking privilege is the most-often-cited advantage of political office holders, local government leaders also use the more limited perks of their office to send newsletters to constituents and to dominate local news coverage.

In the arena of local government, entry barriers may also be embedded in the "constitutional rules" that structure local political economies (Oakerson and Parks 1988). Rules governing the incorporation of new communities, the annexation of existing ones, and the types of services local government must supply—all affect the range of competition local government officials face and the opportunities for entrepreneurial activity (Burns 1991). The degree of fragmentation and diversity in service offerings in the local area can also affect the incentives and actions of incumbent politicians and the potential for entrepreneurial activity (see Schneider 1989).

The manipulation of entry barriers to limit challenges to incumbents has been most widely studied in terms of Congress, but the effort to protect political incumbency occurs at all levels of government. How local political entrepreneurs overcome existing entry barriers and the extent to which they then try to erect new ones is a critical dimension of a theory of entrepreneurship in local government.

Entrepreneurs and the Collective Action Problem

Organizing collective action in any domain requires that the well-known problems of creating and maintaining organizations be overcome (Frohlich and Oppenheimer 1978; Moe 1980; Olson 1965; Taylor 1982). The stereotypical private sector entrepreneur works alone or in a small organization, thus solving collective action problems by avoiding collectivities. However, a public sector entrepreneur is much more likely to need a collective *group* foundation to survive and prosper in the political marketplace.

Those political entrepreneurs acting in the realm of electoral politics must use rhetoric, the manipulation of public policy goals and the distribution of selective incentives to identify and mobilize a winning electoral coalition. In this electoral domain, in addition to the problems of group formation and maintenance, the entrepreneur faces biases in the distribution of resources and the generation of selective incentives necessary to create new alliances in local politics (see, esp., Stone 1989).

Second, compared to the private sector entrepreneur, public entrepreneurs may be forced to deal with large-scale formal organizations. Lewis argues that public entrepreneurship *begins* in an organizational setting and that the public entrepreneur develops a career both using, then ultimately transforming, the practices of large-scale public organizations: "The public entrepreneur, somewhere during his career, comes to understand that the *large complex public organization is the most powerful instrument for social, political and economic change in the political universe*" (1980, 238; emphasis original). Recent work on "intrapreneurship" (e.g., Pinchot 1985) shows how private sector corporations try to meld entrepreneurs into a complex organizational environment. Similar developments may be occurring in public bureaucracies. While, more generally, we believe that the possible avenues for public entrepreneurship are more diverse than the managerial–bureaucratic route with which Lewis is concerned, a fundamental difference may still exist between the public and private sectors in the size of the organizations that entrepreneurs face as they push their innovations and policies.

ENTREPRENEURS IN LOCAL GOVERNMENT

In local governments, political entrepreneurs can be high-level unelected leaders, such as city managers; elected politicians, such as mayors or members of city councils; leaders of established interest groups; or creators of new groups. These political entrepreneurs develop new and innovative policies and galvanize otherwise difficult-to-organize, dispersed citizens to support their policies.[5]

Others have discussed the role of bureaucrats and managers as entrepreneurs in government, most often at the national level of government. At the local level, Oakerson and Parks (1988) argue that public entrepreneurship often occurs in the context of professional associations that link managers across different local governments. Clearly, much entrepreneurial activity concerning local service efficiency is managerial—especially with regard to the introduction of new technologies within a given service domain.

In addition to this type of managerial entrepreneurship, elected politicians can also play critical entrepreneurial roles. One type of local political entrepreneur active in recent years focuses on the politics of growth. These growth entrepreneurs can be divided into several subclasses. First there is the progrowth politician who advocates using the vast array of traditional tools of local government (such as tax abatements and debt financing) to lure business firms. These politicians had their greatest successes in the 1970s and early 1980s. But eventually, what has been called the "market for jobs" became so fully developed that most opportunities for political entrepreneurs advocating these traditional tools were identified, and entrepreneurial profits bid away, in the intense interlocal competition for economic development (Milward and Newman 1989; see Schneider 1992). Indeed, the use of many of the traditional progrowth tools of local government has been so routinized that they are now handled by a cadre of bureaucratic managers (e.g., Blair, Fichtenbaum, and Swaney 1984; Eisinger 1988). This process replicates that of innovation in private markets: competition leads to the dissipation of initial high entrepreneurial profits, and entrepreneurial leadership is replaced by routine bureaucratic processes handled by managers.

In contrast to the progrowth politician advocating the traditional tools of economic development, a new class of growth entrepreneurs is now emerging. Politicians in subnational governments are now advocating a whole new range of what Eisinger (1988) calls demand-side policies to promote economic development. This evolution in policy tools is congruent with a theory of entrepreneurship in local government. As the last set of innovative tools became routine, the payoffs for these now traditional policies dwindled. In turn, a new generation of political entrepreneurs create and experiment with policies with potentially higher rewards. But to the extent that these policies are successful, political entrepreneurs will continue entering the local market advocating similar policies. Ultimately, the political profits attached to these innovative policy tools will decline; and they, too, will become standardized bureaucratic products. While this new generation of entrepreneurs advocating innovative approaches to economic development are pushing the boundaries of existing policies, they still support local growth. Even more radical challenges are emerging from antigrowth entrepreneurs—entrepreneurs whose political careers are built on questioning the very value of growth itself (Logan and Molotch 1987; Logan and Zhou 1980; see Schneider and Teske n.d.).

Yet another type of local political entrepreneur focuses more directly on services and taxes, emphasizing efficiency-enhancing reforms. Borrowing from Clark and Ferguson (1983), many of these "new fiscal populists" fit the profile of an entrepreneur; that is, they are politicians who challenge the existing service/tax arrangements in local governments by advocating policy reforms to reduce local taxes and improve the efficiency of local government services. Certainly, there were many such entrepreneurs in local government in the mid-to-late 1970s, but it is empirically unclear how open this field is at present for new entrepreneurs of this type.

In each case, the successful entrepreneur needs to create a political coalition by which to challenge the status quo. The new fiscal populist seized on dissatisfaction with rising local taxes and the middle-class predisposition for the norm of efficiency to challenge existing political/bureaucratic alignments. Often, these new fiscal populists not only challenged the political leadership in their communities but actually changed the rules of the game. For example, through initiatives and referenda these entrepreneurs created the need for extraordinary legislative or voting processes to allow higher taxes. In short, these entrepreneurs created and maintained a political coalition that challenged existing local tax policies and transformed the institutional rules of the politics of local taxes.

Similarly, to be successful, the antigrowth entrepreneur must fracture the existing progrowth political alliance found in many communities and create a new political orientation toward growth. Neighborhood and community organizations that are faced with rapidly accumulating costs of growth usually provide the organizational basis for the antigrowth entrepreneur. These increasing costs provide opportunities for alert antigrowth entrepreneurs to challenge the position of the progrowth politicians who have long dominated local politics (e.g., Elkin 1985; Logan and Rabrenovic 1990; Mollenkopf 1983). This alternate organizational base is often mobilized as the antigrowth entrepreneur introduces a new dimension in local debates over growth, a dimension that emphasizes the use value of land (Logan and Molotch 1987), replacing the traditional concern for its exchange value (see Schneider and Teske n.d.).

STALKING THE LOCAL POLITICAL ENTREPRENEUR

No simple theory exists to explain entrepreneurship in the private market. It follows that a theory of *political* entrepreneurship will necessarily be complex, drawing on work from a variety of perspectives and disciplines. We have developed a core set of ideas that must inform any theory of public entrepreneurship. These concepts include political profits (affected by the extent of entry barriers and the strategic use of information), challenges to entrepreneurs rooted in collective action problems, and the dynamic effect of entrepreneurs on political equilibria.

We believe that most components of our theory are empirically testable. For example, local governments in metropolitan areas can be modeled as interacting in a dynamic Tiebout-like game, in which they must fashion their services and taxes in a manner that makes them competitive with other cities (Peterson 1981; see Schneider 1989). A successful local entrepreneur can change the results of this competition, for example, by introducing more efficient services that affect the competitive position of a city relative to its neighbors. Over time, neighboring cities must respond, changing their own mix of services and taxes to reflect the lower taxes or better services a successful entrepreneur introduces. The extent to which these dynamic (and measurable) changes occur can serve as a test of the effects of local entrepreneurs in the local market for public goods. Similarly, the effects of entrepreneurs on intracommunity equilibria can be explored. For example, by integrating game-theoretic models of the interplay between incumbents and challengers (e.g., Harsanyi 1967–68; Milgrom and Roberts 1982) with Riker's insights and with the literature on the effects of the structure of local government on policy responsiveness (e.g., Grofman and Lijphart 1986; Morgan and Pelissero 1981), specific and testable predictions about the development of policy positions adopted by entrepreneurs and incumbents can be derived.

While we are moving ahead on these fundamental research questions, our present goal is more modest: we limit our empirical analysis to identifying the conditions that affect the emergence of entrepreneurs, concentrating on the first two elements of our theory. We thus address the emergence of entrepreneurs before studying in detail their specific strategies and their policy impact. Specifically, we argue that entrepreneurs are more likely to emerge when there are factors that make entrepreneurial profits more likely and where local conditions facilitate a solution to collective action problems.

To test these relationships, we surveyed municipal clerks in more than fourteen hundred suburban communities across the nation to identify the distribution of political entrepreneurs.[6] Municipal clerks are knowledgeable about the politics and political history of their communities, and we believe this makes them more-than-credible respondents.

Since asking city clerks to identify local leaders who are "alert to disequilibrium" or who engage in "creative discovery" stretches the norms of language (even if the phrasing would be more theoretically pure), we asked clerks whether, during the past several years there had been any individual in their community whose "policy proposals and political positions represented a dynamic change from existing procedures."[7] We received 963 responses, of which 257 (26.7%) nominated someone as a political entrepreneur.[8] Of these entrepreneurs, 43% were mayors, 26% were members of the city council, 23% were city managers or bureau chiefs, and the remainder were from business or from other private activities.[9] We found no significant regional pattern in the distribution of entrepreneurs.

We analyze the emergence of a local entrepreneur as a dichotomous (0, 1) variable (1 = entrepreneur reported in the community) and use probit analysis. In the analysis that follows, we thus identify objective local conditions that affect the probability of an entrepreneur's being found in a given community. Because our questionnaire asked clerks to name entrepreneurs who had emerged over the last several years, we use specific conditions of communities in the early 1980s to predict the emergence of an entrepreneur.

To correct for regional differences in the distribution of community conditions, all the independent variables have been adjusted by metropolitan means and standard deviations (that is, they have been turned into z-scores by Metropolitan Statistical Areas). Through this transformation, the independent variables reflect the standing of a community relative to its neighbors (Collver and Semyonov 1979).

The Effects of Local Conditions on the Probable Emergence of an Entrepreneur

We model the probability that an entrepreneur emerged as a function of local fiscal conditions, local budgetary patterns, and local demographic conditions. The fiscal wealth (tax base) of a community is a fundamental resource that should motivate potential political entrepreneurs. A strong tax base gives communities a wide range of policy options for the entrepreneur to pursue. Since communities with a strong tax base can provide good services at a moderate tax rate or moderate services at a low one (see Schneider and Logan 1982), a strong tax base represents a public resource entrepreneurs can use in pursuing their vision of the future.[10]

A second fiscal variable, the comparative local tax rate, is more likely to reflect the community's potential support for an entrepreneur. When local taxes are high relative to the taxes of other communities, an opportunity may exist for an entrepreneur to advocate innovations that increase the efficiency of local services, limit expenditures, or redirect patterns of growth that residents might associate with higher taxes. Thus, we hypothesize that both a higher comparative tax base and a higher tax rate will increase the probability that an entrepreneur will emerge in a given community.

Entrepreneurs will also be attracted by the possibility of redeploying local budgets to achieve the policy goals they prefer. Following Peterson (1981), we divide local expenditures into three categories: developmental, allocational, and redistributive.[11] According to Peterson, within the types of expenditures that constitute local budgets, allocational expenditures are the most free of political limits. In Peterson's analysis, cities have little leeway in their expenditure decisions over redistributive services (which they must avoid) and over developmental expenditures (which they must pursue in a nonpolitical professional manner). This leaves allocational expenditures as the domain over which local politicians have the most influence. From our perspective, high allocational expenditures create slack resources that increase public sector opportunities for entrepreneurial profits. These slack resources can be most easily reallocated by the entrepreneur to pursue preferred policies.

The probability of an entrepreneur's emerging should also be related to the demographic makeup of the community, because such conditions reflect the relative ease with which entrepreneurs can organize the citizenry into collective political action. One important variable affecting the ease of organizing for collective action is the stake that individual citizens have in local tax and service issues. We hypothesize that communities with a greater concentration of renters (individuals without a location-specific investment in property) will be harder to organize than communities with a larger concentration of more firmly anchored homeowners (Cox 1982).[12]

As we have noted, progrowth and antigrowth politics are increasingly important conflicts in local communities. Rapid growth can strain existing services, increase pressure on taxes, and mobilize local citizens who see the quality of life in the community changing. We thus hypothesize that rapid growth in a community will increase the likely emergence of a political entrepreneur.

We also believe that the growing racial heterogeneity of suburban communities may create the conditions for political entrepreneurship by providing a base for new political demands or by countermobilizing existing residents to the demands of a growing racial minority population. Specifically, we hypothesize that a larger black population can add a new racial dimension to local politics that can increase the probability of local entrepreneurial political behavior. We also include a suburb's distance from the central city as a control variable, since this condition is often directly correlated with a variety of economic and social conditions of suburban communities (Dye and Garcia 1978).

Table 1 shows the results of a probit estimation of the effects of these conditions on the emergence of a local entrepreneur.[13] In the probit estimation, the most important variable affecting the emergence of an

TABLE 1

Probit Estimates: The Effects of Local Conditions on the Emergence of a Local Political Entrepreneur

VARIABLE	COEFFICIENT	SE	PROB > |T|
Fiscal conditions			
Tax rate	.102	.059	.086
True value	.064	.063	.311
Expenditures			
Allocational	.151	.063	.017
Redistributive	−.092	.061	.134
Developmental	−.046	.061	.454
Demographic conditions			
Percentage renting	−.127	.061	.037
Growth	.104	.058	.072
Percentage black	.105	.058	.098
Distance	−.058	.060	.335
Constant	−.619	.054	.000

Note: N = 635; chi squared = 23.43 with nine degrees of freedom (p = .005). Percentage of communities with entrepreneur = 27.5%. All independent variables are z-scores, i.e., they are standardized by regional means and standard deviations. *Tax rate* is the effective local tax rate; the tax base is the equalized value of local property per capita. *Allocational* expenditures are the standardized per capita expenditures on allocational functions, 1982; *redistributive* expenditures are the standardized per capita expenditures on redistributive functions; *developmental* expenditures are the standardized per capita expenditures on developmental activities. See n. 11 for full description.

Sources: State and county tax reports; U.S. Bureau of the Census, 1982 *Census of Governments*, Finance, File A; 1980 *Census of Population* (Washington: Government Printing Office).

entrepreneur is the size of slack budgetary resources measured by allocational expenditures. The importance of this budgetary variable is followed by the size of the rental population, which, we argue, makes the collective action problem more difficult for the political entrepreneur. Using the appropriate one-tailed significance test, we find that the disruptions of population growth and increasing racial diversity also increase the probability of finding an entrepreneur. Finally, note that a higher tax rate does increase the probability with which an entrepreneur will be found—high costs being associated with opportunities for entrepreneurs. However, the tax base variable is not related to the emergence of an entrepreneur at any acceptable level of statistical significance.[14]

Table 2 reports the effects of each individual variable on the probability of finding an entrepreneur. Keeping all other variables at their mean (since they are z-scores, the mean is 0) and looking at the effects of a variable set at one standard deviation below, and then one standard deviation above, the mean, we can get a further idea of the importance of each variable. The range for each variable alone is relatively narrow. Allocational expenditures has the largest effect in changing the probability with which an entrepreneur is found, with a change in the probability of finding an entrepreneur increasing from .22 to .32. This is followed by percentage renting, growth, and percentage black. While the range for each variable individually is narrow, the cumulative effect of all the variables we measure is considerable. If all the conditions in our model are set at one standard deviation from the mean to reduce the probability of finding an entrepreneur, the likelihood of finding an entrepreneur is only .07.[15] When we reverse the signs on the variables to increase the likelihood of finding an entrepreneur as a function of the conditions we measure, the probability increases substantially, to almost .59.

ENTREPRENEURS IN THE LOCAL MARKET FOR PUBLIC GOODS

Entrepreneurs play an important role in the American policy process. While the effect of entrepreneurs has been most widely studied at the national level, we believe that many opportunities for entrepreneurial behavior exist in the thousands of subnational governments in the United States. We further believe

TABLE 2

Sensitivity of Probit Estimates: The Probability of Finding an Entrepreneur, Given a One-Standard-Deviation Change in Local Conditions

VARIABLE	−1 S.D.	+1 S.D.
Tax rate	26.5	30.2
True value	24.7	28.9
Allocational expenditures	22.0	32.0
Redistributive expenditures	29.8	23.8
Developmental expenditures	28.3	25.9
Percentage renting	31.0	22.8
Growth	23.4	30.3
Percentage black	23.6	30.1
Distance	28.7	25.9
All variables	7.2	58.7

Note: Sample mean = 27.5

the concept of entrepreneurship should not be limited to a few figures with national reputations. In addition, most studies of political entrepreneurs have focused on the actions of a single entrepreneur facing a specific policy situation. Neither the motivations of entrepreneurs nor why entrepreneurs emerge has been investigated in a systematic manner. As a result, most existing studies of political entrepreneurship are not cumulative. An overarching theoretical perspective on the political entrepreneur is needed, a perspective that will allow individual studies of entrepreneurs to be tied more closely to each other and that will increase our understanding of how entrepreneurship is embedded in the systemic process of political change. We present the beginnings of a theory that synthesizes elements of an economic approach to entrepreneurship with existing work on political entrepreneurs.

It is important to emphasize that we believe a *synthesis* of political and economic theory is required. The study of political entrepreneurs will be ill served by the wholesale importation of economic theories of the private sector entrepreneur. The study of public entrepreneurship requires different emphases and focal points from the study of private sector entrepreneurs. For example, a fully developed theory of political entrepreneurship will require the definition of an expanded utility function for the entrepreneur. In particular, compared to the analysis of private sector economic entrepreneurs, the study of political entrepreneurs will require a more complex approach to profits, with an explicit concern for the role of private information and entry barriers. The study of political entrepreneurs will also require a much fuller investigation of the relationship between the strategies of the entrepreneur and problems of collective action than is the norm in economics.

While most studies of political entrepreneurs begin with an entrepreneurial politician, a more complete theory of political entrepreneurship must begin one step back: What motivates the entrepreneur? What conditions systematically affect the probability that an entrepreneur will be found? While our indicators are not as neat as the monetized concept of entrepreneurial profits of economic theory, our data show that slack public sector resources attract entrepreneurial politicians. These resources provide opportunities for the entrepreneur to garner the local public sector's equivalent of profit. Similarly, given their concern for individual behavior, economic theories of the entrepreneur are not usually concerned with collective action. But our evidence shows that problems of collective action strongly affect the emergence of local political entrepreneurs.

In short, our theoretical concern for an integrated theory of political entrepreneurship provides conceptually richer insights into the role of entrepreneurs in the policy process than do the separate theoretical traditions in economics and political science upon which we draw.

Notes

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1. We do not know the actual size of the local population of potential entrepreneurs. Following the most recent literature on leadership, we believe the population to be much larger than implied by the biographical case studies on entrepreneurs, which tend to glorify their subjects as heroic, and which almost by definition makes them uncommon.

2. For a fuller description of the local market for public goods, see Schneider 1989.

3. For a recent summary of work building on Elder and Cobb, see Milward and Laird 1990.

4. Information asymmetries and the strategic revelation of information has become a topic of interest in the study of legislatures (see, e.g., Austen-Smith and Riker 1987; Gilligan and Krehbiel 1990). The insights contained in such studies of committees and legislatures, set in a particular institutional arena, do not easily carry over to the study of the entrepreneurial process with which we are concerned.

5. We are specifically interested in the role of individual political entrepreneurs in the development of local policies. However, these individuals are just one class of entrepreneurs found in the local market for public goods. For example, building on the work of Charles Tiebout, we are exploring how mobile households and business firms can also play entrepreneurial roles in the development of local policies. Moreover, political entrepreneurs can have radically different goals. As we argue later, some are concerned with fast growth, others with slow growth, and yet others with taxes. In other work, we are exploring differences between types of entrepreneurs; here we deal with the entire set of individual entrepreneurs.

6. We began our sample construction with all incorporated suburbs with populations greater than 2,500 in 1970 located in the 100 largest metropolitan regions of the United States (but no more than three metropolitan areas per state). This produced a set of over 1,400 suburbs in 55 metropolitan regions, of which clerks in 963 communities responded to our survey. However, limits on data availability further reduced the number of cases for analysis.

7. We recognize that this operational definition is fairly broad, encompassing entrepreneurs who change local politics, alter equilibria, develop and implement new ideas or policies, and so on. However, our operational definition is virtually identical to that developed by Roberts (1991), who has presented perhaps the most recent comprehensive review of the literature on entrepreneurship from a political science perspective. In our on-going research, using a combination of follow-up surveys, objective data from various census reports, and selected case studies, we are exploring in more detail what these entrepreneurs have done (with particular concern for the theoretically central issues of the role of entry barriers and the dissemination of information), how their actions and positions actually differed from previous policies, and what effects the entrepreneurs have had on local services and taxes.

8. Since this is the first large-scale survey of suburban municipalities, it is impossible to say whether the 27% rate of entrepreneurship is high or low. However, in a comparative study of economic development strategies in close to two hundred cities, Clarke (1990) finds "innovative" programs in 5%–21% of the cities surveyed. Since her numbers are for individual programs, it is not unreasonable to assume that a joint set of programs that might define an entrepreneur will be found in the proportion of cities we report.

9. Clerks were informed that the survey was part of a large nationwide study of political entrepreneurs. The questionnaire began with a series of questions about the local

government structure and local politics. Clerks were then asked to name someone who met our definition of an entrepreneur. The clerks who "nominated" an entrepreneur were then asked a series of open- and closed-ended questions about the entrepreneur's activities. Clerks could nominate one or more entrepreneurs in the 1980s. They were also given the opportunity to name any entrepreneurs from the 1970s.

10. The tax base is measured as the per capita true value of property in the community. The tax rate is the effective tax rate. These measures are adjusted for differences in assessment practices, using state and county reports.

11. Expenditures on business-related "developmental" services are measured by combining local per capita expenditures on streets and highways, sewers, sanitation, water transport, water supply, gas, electricity, and transit. Redistributive services are defined as health, hospitals, urban renewal, and welfare. Allocational expenditures are for "housekeeping" functions (general administration, general government, financial administration, and buildings) plus public safety (police and fire).

12. Of the variables we use in the following analysis, percentage renting and percentage black are from the 1980 census of population. Population growth is the 1970–80 rate of growth. Since 1990 census data are not yet available for individual communities, these are the only data available. Moreover, Donovan and Neiman (n.d.) show that the lagged rate of growth is more important than the current rate of growth in determining local growth politics. Distance is measured as the straight line linear distance between the outer boundary of the central city and the inner boundary of the suburb.

13. The number of cases in the probit estimate is less than the full number of cases for which we have information on the emergence of a local entrepreneur. Only the cases for which we have information on every independent variable is included in the estimation. Missing data on the fiscal variables (which are gathered not centrally, but from state and county reports) account for the elimination of most cases.

14. There is a high correlation (.41) between relative allocational expenditures and relative tax base. This is not surprising—a higher tax base leads to more resources available for expenditures on the relatively more discretionary local services encapsulated in the allocational category.

15. In other words, for variables with a positive coefficient, we estimate the probability of finding an entrepreneur while setting the variable at one standard deviation below the mean. For variables with a negative coefficient, we set the variable at one standard deviation above the mean. To increase the probability of finding an entrepreneur as a function of these variables, we reverse the signs. Note that since all the variables are z-scores, the value of the variable at one standard deviation from the mean is plus or minus one.

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SIZING UP THE PEACE DIVIDEND: ECONOMIC GROWTH AND MILITARY SPENDING IN THE UNITED STATES, 1948–1996

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We examine the relationship between military expenditures and economic growth in the United States from 1948 to 1990, in order to gauge the potential peace dividend. Our main results suggest that military spending is a significant drain on the economy. We then examine the implications of a restructured international system for U.S. military expenditures and their resultant impact on economic growth in the 1990s. Simulations of Democratic and Republican proposals for cuts in defense spending suggest increases in economic output of between 2.5% and 4.5% over the period 1993–96.

> With cold-war expenditure no longer needed, our gigantic government expenditures abroad could be reduced and our international deficit would probably disappear almost overnight. With the dollar no longer overvalued, militant expansion of monetary and credit policy could stimulate capital formation, [or investment,] to utilize the resources released from armament production. . . . America's potential and actual growth rate, far from depending upon war preparations, would be markedly increased by an end to the cold war.
>
> Paul Samuelson, *Economics: An Introductory Analysis*

The recent collapse of the Soviet Union has opened the door for a reconsideration of the role and obligation of the United States in a post–cold war international system. As Paul Kennedy argued, the United States has been on the horns of a dilemma (1987, 532–35). It has extensive political commitments that "require" large resource commitments to the military. At the same time, (relative) industrial decline, coupled with the twin problems of massive external and internal debt have all suggested the economic "fall" of the United States, particularly in terms of declining competitiveness and stagnating economic growth. Furthermore, the increased levels of economic activity and economic productivity of Europe, as well as Asia, have made the commercial world increasingly competitive. The dilemma has the potential of being resolved, however, as diminished military threat poses an opportunity for restructuring U.S. military commitments.

Concurrent with the relative decline of U.S. economic health has been the elimination of the major military threat to the United States. This has brought about significant restructuring of U.S. military policy away from stability based on nuclear deterrence. What will replace the bipolar balance of terror is at present unclear, but it is clear that the total threat to the United States has diminished in remarkable ways over the last few years. This dramatically reduced threat has led to widespread agreement that a rethinking of U.S. defense policy is required. Many have called for a significant downsizing of the U.S. military. President Bush noted in the 1992 State of the Union Address: "Two years ago, I began planning cuts in military spending that reflected the changes of the new era. But now, this year, with imperial communism gone, that process can be accelerated." Paul Tsongas has summarized much current opinion in declaring, "The basic economic reality we face in this country is very simple: the Cold War is over. . . . We can make deeper defense cuts and still maintain the world's strongest military" ("The Democrats on Defense," *Boulder Daily Camera*, 3 March 1992). Everyone seems to agree that the cold war is over; just how much of a dividend a restructuring of U.S. defense posture can produce is still in question.

Defense Secretary Cheney has asserted that the only real peace dividend is peace. On the other hand, many others have put forward the notion that a peace dividend may well provide a needed stimulus to the economy and overall U.S. global economic competitiveness. In one form, this argument targets other social programs as the beneficiary from the windfall, while others suggest that economic output itself will be enhanced. Deficit reduction and the stimulation of private investment also are targets of those wishing to spend the putative peace dividend.

Joshua Goldstein (1988) maintains that every 1% of gross national product (GNP) devoted to military spending "robs" the United States of about ½% of economic growth.[1] A recent International Monetary Fund study of 125 countries from 1972–88 concluded, "While some have argued that military expenditures enhance economic growth, there is no question that more efficient alternative means of using the resources exist that would provide greater benefits to the economy" (Hewitt 1991b, 20; see also Hewitt 1991a).

Results of contemporary research on the United States, however, are not so clear concerning the relationship between military spending (and the like) and economic activity. A number of important studies have addressed the trade-off issue and found evidence supporting the contention that the United States has paid an economic price for its military spending. Russett (1969, 1970) found evidence indicating that defense expenditures displace investment, personal consumption, and state and local government expenditures; Russett's conclusions were based on the results of analysis between defense spending as a proportion of GNP and nongovernment spending proportions, rather than within the context of a macroeconomic model. Russett's later (1970) study found additional evidence supporting the argument that there was a strong bivariate trade-off between defense and welfare spending ratios within the United States; subsequently, he reexamined the trade-off hypothesis using a more sophisticated research technique and data from a longer time period and found no evidence of systematic trade-offs between defense spending and health and education expenditures (1982).

Mintz and Huang (1990, 1991) have studied the relationship between economic growth and defense expenditures over the period 1953–87. Recently, testing for externality effects, they found no statistically significant impact of defense spending on economic growth in the United States (Huang and Mintz, 1991). In their earlier studies, using a two-equation model of growth and investment, they found no direct relationship between military spending and economic growth; but they uncovered an "indirect" relationship between military spending and investment five periods subsequent (at the .10 significance level). Military spending in 1990, for example, would drive down investment levels in 1995. They concluded: "The findings reported in this study reveal the existence of an indirect, delayed effect of military spending on growth. To be more specific, we found that in the long run, lower military spending encourages investment, which in turn promotes economic growth. This effect is not immediate, however. Our analysis shows that it takes about five years for such an indirect tradeoff to begin to become manifest" (Mintz and Huang 1991, 1291).

In another study of defense spending and economic growth in the United States, Atesoglu and Mueller (1990) employed a similar neoclassical growth equation, focusing on elasticities. In this study, the economy is divided into a military and a civilian section, the latter including nonmilitary, governmental spending programs. Atesoglu and Mueller found that the elasticity of GNP vis-à-vis defense spending is inelastic and positive. This implied that growth in defense spending leads *directly* to increases in output.

We reexamine the question of the trade-off in the United States, using data from 1948 through 1990. Like others, we look at the defense–growth issue within a neoclassical economic growth model. However, we focus on the separate size and externality effects of military and nonmilitary expenditures. In so doing, we examine the multiplier effects of military and nonmilitary components of governmental spending to discern their separate impacts on the macroeconomic environment. In this way, we hope to provide a more accurate gauge of the economic impacts of defense reductions in the coming years.

A MODEL OF DEFENSE–ECONOMY TRADE-OFFS

Ram (1986) and many others since have used Feder's (1982) initial model to study the role of government spending in the economy. This is done by assuming that instead of an export and a nonexport sector developed by Feder, the macroeconomy is divided into a governmental and nongovernmental (or private) sector. Following this idea, we assume that there are two sectors contributing to national economic output, P and S, representing the *private* and *state* sectors, respectively. Capital, K, and labor, L, are the main inputs to each sector, although output of the state sector S, is also an input to the private sector.

$$P = F(K_p, L_p, S)$$

$$S = G(K_s, L_s)$$

where F and G are real valued functions; subscripts refer to sectors.

Biswas and Ram (1986) were the first to use this set of assumptions to study the impact of military spending on economic growth by substituting a military and a nonmilitary sector for Feder's original export and import sectors. Ram (1986) was the first to apply these ideas to a state and a private sector. Building on Ram's application, Mintz and Huang decomposed state activity into military spending M and nonmilitary state spending N (1990, 1991; Huang and Mintz 1990). (For a full derivation of the model, see Ward et al. 1991.) The neoclassical growth equation to be empirically estimated is

$$\frac{\Delta Y}{Y_{-1}} = \alpha_0 + \alpha \frac{I}{Y_{-1}} + \beta \frac{\Delta L}{L_{-1}} + \left(\frac{\delta_M}{1+\delta_M} - \theta_M\right)\frac{\Delta M}{Y_{-1}} + \left(\frac{\delta_N}{1+\delta_N} - \theta_N\right)\frac{\Delta N}{Y_{-1}} + \theta_M \frac{\Delta M}{S_{-1}} + \theta_N \frac{\Delta N}{S_{-1}} + \epsilon. \quad \textbf{(1)}$$

National income is represented by Y, investment by I, and labor by L. This equation assumes that the contribution of M and N to growth via the externality (θ) and the factor productivity differential (δ) are not necessarily equivalent and associates unique δs and θs with respective components of M and N, an assumption that can be tested empirically.

The basic idea of the model is that changes in factor inputs (i.e., investment, labor and the various types of government spending) contribute directly to the growth rate of economic output, or national product. This neoclassical assumption is further modified by separating out the size effects of spending patterns from their externality effects. *Size effects* represent the contribution to national product of the overall spending levels. Government purchases, in effect, contribute to national productivity. *Externality effects* refer to the spillover contributions. Government spending on roads, for example, contributes indirectly to the efficiency and productivity of nongovernmental sectors. In general, if government activities developed outside of the market economy contribute to productive capacity they are said to represent an externality effect. (A broader discussion of these effects may be found in Cornes and Sandler 1986; Huang and Mintz 1991 explores the externality effects of defense spending on the U.S. economy using a three-sector production function approach.

We apply this model to the United States because we believe that externalities of defense spending are important in understanding the defense–economy trade-off and that the linkage between the military and the aggregate economy is sufficiently strong to be evident directly. This model also permits us to discern the separate effects of military and nonmilitary spending on economic growth.

A number of theoretical expectations flow from this model. The United States benefited from contributions to industrial infrastructure over the past decades. Accordingly, we expect that the impact of investment on changes in aggregate output should be significantly positive: $\mathcal{H}_1 : \alpha > 0$. Furthermore, we posit that gains in labor inputs will be translated into output gains, that is $\mathcal{H}_2 : \beta \geq 0$.

Ram (1986) found that the size effects of government spending are generally positive. However, military spending is often thought to be inherently nonproductive and a drain on productive resources in mature economies. Accordingly, we posit that military spending will have negative size effects on output while nonmilitary spending programs will generally have positive size effects. Thus (for m, n = military/nonmilitary governmental spending),

$$\mathcal{H}_3 : \frac{\delta_m}{1 + \delta_m} - \theta_m < 0$$

and

$$\mathcal{H}_4 : \frac{\delta_n}{1 + \delta_n} - \theta_n > 0.$$

Military spending programs are also frequently thought to bring spinoff effects as technologies and production processes initially developed for defense applications are brought easily and without separate research and development costs into the private sector. On the basis of this argument, we posit that the military spending of the United States will have positive externalities. Nonmilitary state spending programs are also thought to have positive externalities in some regards, since much state spending is aimed at providing infrastructure (e.g., roads, schools, national defense, and other public goods) that benefit commerce. For i = military/nonmilitary state spending, we hypothesize that $\mathcal{H}_5 : \theta_i > 0$.

Before turning to a description of the data employed and our empirical results, it is important to catalogue, however briefly, some weaknesses of this approach. In adopting a production function approach that assumes an equilibrium between supply and demand, one necessarily glosses over the ability of the economy to function in disequilibrium. We ignore the major mechanisms through which the macroeconomy attempts to deal with disequilibrium: prices, the labor market, the responsiveness of investment to changes in output, and financial markets. A more complete model would provide a description of demand, as well as supply. Additionally, by adopting an economywide description of the United States, we ignore the existence of linkages that may be present in one sector (such as high technology) and absent in another (such as textiles). We also ignore regional differences, differences that may be stronger in some parts of the country and absent, weak, or opposite in the other locales. Available models of open economies are considerably more fine-grained, sophisticated, and advanced than the model presented here. However, the model employed herein is a simple and straightforward tool for evaluating the impact of military expenditures on economic growth, while permitting us to tease out the economic implications of various policy options for reducing defense expenditures under consideration.

DATA

Data measuring GNP, military outlays, government spending, and investment were all taken from the 1986 U.S. Department of Commerce publication, *The National Income and Product Account of the United States; 1929–1982.*[2] These measures were updated through 1990 from the *Survey of Current Business* (Dec. 1989 and 1990, July 1991, and Jan. 1992). Data measuring employed civilian labor force were taken from the U.S. Department of Labor (1989, 1991). All fiscal data were deflated into constant 1982 U.S. dollars using the GNP deflator and are given in calendar years.

EMPIRICAL RESULTS

Table 1 presents the results of the ordinary least squares estimates of equation 1. Studies by Mintz and Huang (1990, 1991; Huang and Mintz 1990) and Atesoglu and Mueller (1990) have argued that multicollinearity is a severe problem in formulations of the sources of growth equation. Accordingly, Atesoglu and Mueller drop consideration of the externality effects from their analysis. Mintz and Huang employ a *k-class* ridge estimator to circumvent collinearity problems and report results for ridge as well as for ordinary least squares estimators. The value of the ridge approach to avoiding problems associated with multicollinearity is, however, widely contested. (See Kennedy 1986 for a discussion of the salient points.) We do not adopt the ridge approach in this study because all of the ordinary least squares parameter estimates are unbiased and highly significant. Increasing their significance by introducing bias would provide no leverage in this instance.

Turning first to the diagnostic statistics, it is clear that the model demonstrates a high degree of fit.

TABLE 1

Military Spending and the Sources of Growth in the United States, 1948–90

	ESTIMATOR: ORDINARY LEAST SQUARES			
COEFFICIENTS	VALUE	ST.E.	T-STATISTIC	PROBABILITY
Constant	−.1053	.0479	−2.1981	.0344
Investment	.6940	.3148	2.2045	.0340
Labor	.6387	.3104	2.0575	.0469
Size effects of				
Military spending	−2.9773	1.2390	−2.4029	.0215
Nonmilitary spending	6.9957	1.4895	4.6966	.0000
Externality effects of				
Military spending	.7369	.2462	2.9934	.0050
Nonmilitary spending	−1.2712	.2888	−4.4017	.0001

Note: Lagrange multiplier tests are serial correlation, $\chi^2 = .464$, $p = .496$; functional form, $\chi^2 = .258$, $p = .612$; normality, $\chi^2 = 1.133$, $p = .568$; heteroscedasticity, $\chi^2 = .508$, $p = .476$; stability, $F(7, 9) = 2.723$, $p = .027$. Diagnostic statistics are $\overline{R}^2 = .7529$; $F(6, 36) = 22.3308$; log likelihood $= 128.6642$.

Over three-quarters of the variance in the rates of economic growth are explained by the model as derived. Moreover, tests of the structural specification of the model support the specification of the model, which shows no evidence of serial correlation, functional misspecification, nonnormality of residuals, or heteroscedasticity. These statistics increase our confidence in the model. Graphic presentation of the fit in Figure 1 also shows that the ups and downs in U.S. economic growth are well captured by this simple neoclassical model.

Turning to the specific coefficients, each estimated coefficient is statistically significant at the .05 level or better. (Robust standard errors are reported.) Most of the coefficients have the predicted sign. We hypothesized that the impact of investment on changes in output would be significantly positive. The estimated coefficient ($\alpha = .694$) suggests that investment has a positive short-term impact on increasing gross national product. Labor inputs also have a positive coefficient, $\beta = .64$.

We posited that military spending has negative size effects on GNP, while nomilitary spending has positive size effects. These speculations are upheld by the empirical analysis:

$$\frac{\delta_m}{1 + \delta_m} - \theta_m = -2.98$$

and

$$\frac{\delta_n}{1 + \delta_n} - \theta_n = 7.00.$$

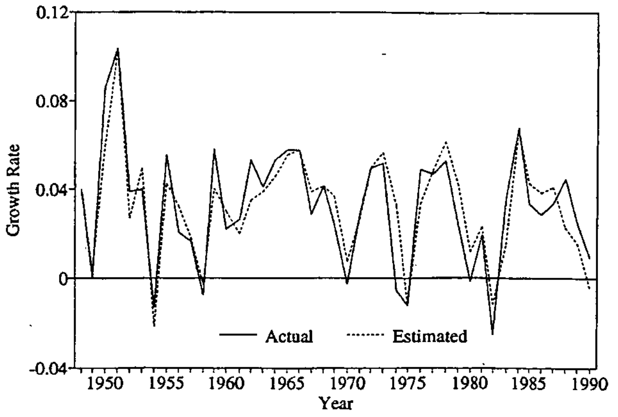
As military spending goes up, economic output will decline; as nonmilitary spending programs are augmented, one may expect positive effects on gross national product. Since military spending is generally smaller than nonmilitary spending, the overall size effect of government spending will be positive; but this hides the fact that military spending not only apparently reduces economic growth rates but may drain resources from nonmilitary spending programs that are likely to have positive size effects on the civilian economy.

Rather than observing positive externalities for both military and nonmilitary governmental spending, we find that the military has a positive externality as predicted ($\theta_M = .74$), while the externality for nonmilitary spending programs is negative ($\theta_N = -1.27$). Thus, it appears that military spending does generate positive spin-offs or externalities for the civilian economy.

Factor productivity of sector *i* is greater than that of the private sector whenever δ_i is positive. Using estimated values from Table 1 to solve for these parameters, we find that $\delta_N = -1.21$ for nonmilitary government spending and $\delta_M = -.69$ for military spending. Taken together, these coefficients suggest that on average, over the entire time span, *government* sectors have lower factor productivity in the United States than civilian sectors.

FIGURE 1

Actual and Fitted Values of Growth Rates in U.S. Gross National Product, 1948–90



SIZING UP THE PEACE DIVIDEND

Having developed and tested a model of the impact of both military and nonmilitary spending on the economy, we are in a position to forecast the impact that changes in U.S. government spending may have on the economy. During 1991 and 1992 there has been intense debate in the defense policymaking community about the appropriate future trajectory of U.S. defense spending.

One major proposal is that of the Bush administration, seen in terms of the revised Five Year Defense Plan (FYDP) put forward by Secretary Cheney in late 1991 and early 1992. This plan calls for defense spending to be reduced by a total of about fifty billion (current) dollars over the period 1992–96, although the level of defense spending in 1996 would be approximately ten billion dollars lower than current levels. This is widely referred to in the press as the "fifty billion" plan, and represents the view of the Bush administration as exemplified in the 1992 administration budget proposals that while some cuts in defense spending may be undertaken, the international environment remains threatening and uncertain, so that widespread cuts would be imprudent.

William W. Kaufmann and John D. Steinbruner have offered a timely criticism of this point of view in *Decisions for Defense: Prospects for a New Order* (1991). This volume offers a detailed analysis of the force structure and financial requirements of various strategic and operational postures that might guide U.S. defense policy in the years ahead. Kaufmann and Steinbruner offer an alternative conception, called *cooperative security*, that served as the basis for a different set of projections of defense spending, projections considerably lower than those put forward by the Secretary of Defense. The basic idea behind the Kaufmann and Steinbruner plan is to rethink and restructure U.S. defense planning on the basis of a collective arrangement among many national governments around the globe, rather than trying to use the defense budget to perpetuate U.S. military preponderance unilaterally. In much the same way that European Ministries of Defense are beginning to discuss a European Defense Union (Fontanel and Smith 1990), this plan would imply a subordination of U.S. traditional objectives of containment and deterrence to a cooperative strategy of engagement that would necessarily involve other important global powers.

According to Kaufmann and Steinbruner (in subsequent testimony to the U.S. Senate Appropriations Committee, 19 February 1992), the basic plan is premised on the notion that "participating military organizations are all on the same side, are all defensively configured, and are all primarily committed to providing mutual reassurance. This requires them to systematically limit offensive capabilities that might support ground invasions or might undertake long-range bombardment to achieve some political objective" (Kaufmann and Steinbruner 1991, 70–71). This type of arrangement would include the notion of an inhibition of firepower concentrations—that is, conventional arms limitations; a geographical restriction of the location of force concentrations; tactical air restrictions; full disclosure of research activities affecting weapons technologies; maintainence of small "residual" nuclear forces; and widespread and consolidated weapons export controls. Such an arrangement would require the participation of the United States, the Commonwealth of Independent States (especially Russia), the United Kingdom, France, Germany, Japan, and China at a minimum. Cooperative security permits Kaufmann and Steinbruner to tease out the force implications of such a strategy and from there to derive the budgetary implications of such a strategic restructuring over the next five to ten years.

These two different visions of the strategic future have been in considerable conflict among the Washington decision makers. The Pentagon's 18 February draft of the *Defense Planning Guidance for the Fiscal Years 1994–1999* criticizes the cooperative security approach and offers the strategic vision behind the administration's then-current FYDP. The guiding principal is "to maintain the U.S. position as the only military superpower in the world."[3] In addition to preventing the emergence of a major power dominating Western Europe, the territory of the former Soviet Union, East Asia, or Southwest Asia, this new plan promotes a military balance that will convince "potential competitors that they need not aspire to a greater role or pursue a more aggressive posture to protect their legitimate interests"; this will putatively assure that no rival superpower emerges in the military realm: "We will retain the pre-eminent responsibility for addressing selectively those wrongs which threaten not only our interests, but those of our allies or friends, or which could seriously unsettle international relations."[4] In short, this policy would reject the ideas of Kaufmann and Steinbruner and pose a distinct alternative not based on a collective or cooperative internationalism as the foundation for U.S. defense policy. In so doing, the required force structure would continue to be substantial and the potential for reduced military spending would be modest.

In these two basic plans, we see not only different strategic postures, but also budgetary implications that are quite distinct. In large measure, the administration plan has been backed by Republicans, and the Kaufmann–Steinbruner plan (or a version of it) has been backed by Democrats. Table 2 presents the specific top-line proposals in each of these two plans for annual defense spending 1992–96. It seems useful to tease out potential economic implications of these two different scenarios. To do so, we used estimated coefficients for equation 1 to structure the simulation. Since the formulation is dynamic, it produces growth rates and GNP for future periods, given exogenous data on investment, government spending, and labor. We used recent historical growth rates for these variables (the decade from 1980–89) but supplemented these by actual data for 1991 where available.

TABLE 2

Comparison of Five Year Defense Plan with Cooperative Security, 1992–96

YEAR	FYDP	COOPERATIVE SECURITY III
1992	295.2	295.2
1993	285.9	275.5
1994	282.0	256.8
1995	285.8	239.0
1996	289.1	222.4

Note: These data are for outlays, given in billions of current dollars. The Bush FYDP figures are from the Office of Management and Budget estimates of Total 050 expenditures. The Cooperative Security III scenario data appear in Steinbruner 1992. These data are a slightly accelerated version of the cooperative security option described in Kaufmann and Steinbruner 1991, 74ff).

(Complete details on the data and the simulations are available on request.) We used actual data on investment for 1991, from the *Economic Report of the President* Bush (1992), as well as official projections of government spending for the period 1991–93 in the same source. During this period, investment grew by 1.4% per annum, while the labor force grew by 1.7% per annum. During the same period, the growth rate of government spending was 2.9%.

Our simulation compares three defense spending scenarios. The first of these is a benchmark using the 1991 spending levels for each year until 1996. Our second defense spending scenario is that presented in the Bush–Cheney FYDP as of February 1992 as discussed in congressional hearings and summarized by the Office of Management and Budget. Our third military spending scenario is the plan known as Accelerated Cooperative Security III, a plan proposed by Kaufmann and Steinbruner in February 1992.

We also examine three basic options of what to do with the putative "peace dividend." First, we explore what would happen to the economy if savings from the defense reductions could be spent by other government programs. Second, we explore the implications of the so-called "Firewalls Bill" to prohibit savings in military spending from being used by other government programs; rather, the savings would simply disappear from the government budget and go toward deficit reduction. Our third scenario would take the savings from defense down-sizing and put them directly into private investment. This last option is based on the widely held notion that defense spending "crowds out" private investment. (Mintz and Huang 1990 offer a summary of this argument; see also Paul Samuelson 1964, 785). Private investment has been dropping in constant terms over the period from 1989 to the present. This scenario attempts to gauge the impact of a stimulation of investment as a result of defense downsizing. We note that the 1992 budget is the same in each scenario; it is only in 1993 that the implications of any of the proposed budgets will be felt upon the U.S. economy.

Table 3 presents the projected level of annual output (GNP) based on each of the three defense budgets in each of the three savings options over the period 1993–96. These projects are given in constant billions of 1982 dollars. Several things are striking about these results. First, it should be apparent that without a reduction of U.S. defense spending, real economic growth will be miniscule in the next few years, even assuming a recovery of investment to historical growth levels and an expanding job market. Projected 1996 GNP (in 1982 dollars) is only 1.5% higher than projected 1993 gross national product, under the benchmark scenario of no change in defense spending over the 1991 program targets. We note that actual national income has grown by an average of less than 1% per annum every year since 1989 (Bush 1992). Second, by using savings in defense spending to reduce the deficit, one can begin to see substantial effects on economic output, even in the short run, with either reduction plan, bringing about greater levels of economic growth. Thus,

TABLE 3

Growth Implications of a Peace Dividend, 1993–96: Projected Gross National Product in Billions of 1982 Dollars

USE OF PEACE DIVIDEND	1993	1994	1995	1996
Civilian government programs				
No change over 1991	4210.04	4198.14	4234.10	4275.54
FYDP	4218.87	4228.11	4274.39	4319.54
Cooperative Security III	4223.16	4245.02	4305.54	4367.62
Deficit reduction				
No change over 1991	4210.04	4198.14	4234.10	4275.54
FYDP	4218.87	4224.23	4264.06	4303.94
Cooperative Security III	4223.16	4242.66	4293.15	4341.99
Investment				
No change over 1991	4210.04	4198.14	4234.10	4275.54
FYDP	4222.50	4241.94	4299.63	4358.42
Cooperative Security III	4223.16	4248.81	4320.49	4400.81

whether a Democrat–sponsored cooperative security strategy is adopted or whether a down-sized military sculpted after Pentagon plans for the maintainence of U.S. military preponderance is adopted, the U.S. economy stands to benefit in the short run, with short-term growth rates higher for each reduction scenario. If defense savings can be used to increase investment (following the crowding-out thesis), substantial increases over current trends in output can be expected. However, even these drastic changes in the trajectories of defense spending and investment only suggest that real growth in GNP is going to be less than 2% a year over the foreseeable future. Finally, it is clear that the accelerated cooperative security option consistently has a greater stimulative effect on the U.S. economy regardless of what, if anything, is done with the savings. By 1996, U.S. economic growth under the cooperative security option will be close to 2% per annum, while the 1995–96 growth rate for the Bush–Cheney FYDP is projected to be 1.4% and under the no-change scenario, it would be about 1%.

SUMMARY

This research provides a number of interesting implications for the literature on the defense–growth trade-offs in the United States and for the current debate about the peace dividend.

First and foremost, consonant with theory and speculation suggesting the nonproductive nature of military spending in industrial societies, we find strong evidence that military spending detracts from economic growth. Recent studies have pointed away from a negative link between military spending and economic growth in the United States (Atesoglu and Mueller 1990; Congressional Budget Office 1990; Huang and Mintz 1990; and Mintz and Huang 1990 and 1991). Our results suggest that such a linkage not only exists but is composed of two distinct parts. On the one hand, we find that the size effect of military spending on the economy is negative. On the other, it seems clear that the military spending programs are able to generate spin-off benefits for private sectors of the economy (i.e., have positive externalities) independently of the size of military spending vis-à-vis total output. Furthermore, we have demonstrated that both military and nonmilitary governmental spending have lower factor productivities than the private sector of the economy, indicating that both aspects of government spending are less efficient than the private sector. Finally, we demonstrate that under each of the policy scenarios of reduced military spending, it is reasonable to expect a small but nontrivial boost to the level of gross national economic product by 1996 compared to a scenario of no reduction. This is consistent with the finding by Mintz and Huang (1990 and 1991) that the linkage between investment and growth may be a powerful conduit of the relationship between military spending and growth; our simulations suggest that by stimulating investment with the peace dividend one gains the largest macroeconomic benefit.

With respect to the current political debate over appropriate levels of military expenditures in the near-term future and the "peace dividend," these scenarios indicate that the consequences of the resolution of this debate have important implications for economic growth, especially for how it is allocated. For instance, the Democratic proposal would result in a GNP 1.6% higher by 1996 if the peace dividend is spent on deficit reduction, 2.1% higher if it is channeled to other aspects of government expenditures, and 2.9% if it is directed into investment. Corresponding numbers for the Bush–Cheney proposal are .6%, 1.0%, and 1.9%, indicating that the overall magnitude of the cut is also critical, supporting the assertions of Kennedy (1987) that maintaining a hegemonic military position carries with it economic consequences.

It turns out that cutting the defense budget is a question that is political *in the extreme*. Not only does the current discussion come in the throes of a presidential campaign, but also the current downsizing has local, as well as national and international, implications. Gordon Adams notes that "Congress is caught between a rock and a hard place. The rock is the desire for a peace dividend. The hard place is jobs in your district."[5] It is clear that local dislocations will attend to specific defense cuts. For example, the local economy around Monterey, California, will not be the same after Fort Ord is shut down. However, the results of our analyses suggest that on balance the U.S. economy will be stronger with defense cuts than without them. Larger cuts appear to help more, whether employed to offset the deficit, provide resources for civilian governmental programs, or stimulate capital formation. While it is politically challenging to support cuts that will affect local jobs, it does seem clear to all concerned that the military threat faced by the United States is significantly diminished. According to widespread accounts, the economic challenges faced by the United States continue to multiply both in number and complexity; these are made clear in the most recent *Economic Report of the President* (Bush 1992). Given this conjuncture of decreased military threat and increased economic threat, the downsizing of the military that seems to make strategic sense may well carry with it economic benefits that are substantial. However, a strong political will also seems necessary to obtain the peace dividend.

Notes

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1. See also Joshua S. Goldstein, "How Military Might Robs an Economy," *New York Times*, 16 October 1988.

2. We thank Alex Mintz for making the National Income and Product Account data available to us.

3. "Excerpts From Pentagon's Plan: Prevent the Re-Emergence of a New Rival." *New York Times*, 8 March 1992.

4. Ibid.

5. Quoted by Helen Dewar, "With the Cold War Won, Jobs Are Being Lost," *Washington Post*, 14 February 1992.

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CONTROVERSY

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EXPLAINING SELF-DEFEATING FOREIGN POLICY DECISIONS: INTERPRETING SOVIET ARMS FOR EGYPT IN 1973 THROUGH PROCESS OR DOMESTIC BARGAINING MODELS?

How should we explain why a state sometimes adopts a foreign policy in one region that interferes with its concurrent policies elsewhere? In their article in the March 1989 issue of this Review, *Stewart, Hermann and Hermann proposed a three-level process model of foreign policy to explain such Soviet behavior towards Egypt in 1973. The analysis has continuing interest because it interprets the puzzling behavior as a manifestation of general problems of information processing in making foreign policy choices. Richard Anderson suggests that a two-level model of domestic bargaining better accounts for the causal sequence in Soviet-Egyptian relations and is in general more parsimonious. Margaret and Charles Hermann defend their substantive analysis and argue in any case for the complementarity of process and bargaining approaches.*

COMMENT

Why does a state sometimes adopt a foreign policy toward one country or world region that impairs the prospects for success of policies concurrently followed by the same state toward other countries or world regions? Stewart, Hermann, and Hermann (1989) offer a process model of foreign policy to explain one case of this puzzling behavior—the Soviet decision in January 1973 finally to grant Egypt's requests for offensive arms to launch the October War against Israel. This decision remains puzzling because Egyptian use of Soviet arms to attack an ally of the United States could be expected to, and ultimately did, jeopardize the détente that the Politburo was concurrently pursuing.

Even though the Soviet Union is now defunct, both the question raised by the authors and their proposed answer remain worthwhile. Many states adopt self-defeating foreign policies (Snyder 1991). The general model proposed by the authors conditions the observed Soviet behavior not on any peculiar institutional or ideological characteristics of the Soviet Union but on general problems of information processing that might distort foreign policy choices by any national leadership. The authors' model has the particular merit of linking levels of analysis by treating foreign policy as contingent on international events, not determined by them. They explain the Soviet decision by an empirical investigation of two processes: (1) the individual-level process by which each of six Soviet policymakers adjusts his attitude toward the issue in response to new information (in this case, Egyptian president Anwar Sadat's July 1972 decision to expel Soviet military personnel from Egypt) and (2) the group-level process by which policy responds to change in individual attitudes toward Sadat's request for arms. In the authors' model, the Politburo approves delivery of arms to Egypt, despite this decision's incompatibility with détente, because Sadat's expulsion order triggers reconsideration of Soviet Mideast policy without simultaneously motivating a reevaluation of East–West policy.

While flaws in information processing have often drawn attention as candidates to explain self-defeating organizational behaviors, another possibility is domestic bargaining. Belonging to the family of "two-level" or "nested" games (Putnam 1988, Tsebelis 1990), a model of domestic bargaining explains self-frustrating behavior in the international arena as a consequence of interactions among the decision makers in the domestic arena. A domestic bargaining model differs from a process model in analytic focus. While the process model draws attention to the processing of salient international events, the bargaining model draws attention to national leaders' purposive behavior.

One way to evaluate the relative merits of process models and domestic bargaining models is to compare their handling of the evidence in particular cases. In the case of Soviet arms for Egypt, the process model, drawing the authors' attention to Sadat's July expulsion order, induces them inadvertently to commit a causal inversion by attributing an event to an observably posterior cause. I begin with a discussion of the causal inversion. Then I present the alternative domestic bargaining model and lay out its historical reconstruction of the case. I conclude with a discussion of the relative merits of the two models.

THE CAUSAL INVERSION

To explain the Soviet policy reversal in granting Egyptian requests for offensive arms, the authors build a causal chain. They examine change over time in the attitudes expressed toward the Egyptian requests by four Politburo "oligarchs" (Brezhnev, Kosygin, Podgorny, Suslov) and two key advisers (Defense Minister Grechko and party foreign affairs specialist Ponomarev). Sadat's expulsion of Soviet military personnel in early July 1972 causes a shift in the declaratory stand taken by Kosygin in the third quarter of 1972. The resulting disagreement over policy toward the Mideast causes Suslov to realign his public stance with Kosygin's. Realignment among the oligarchs produces approval of the arms deliveries in January 1973.

To detect changes in the oligarchs' and their advisers' expressed attitudes toward the Egyptian demands for arms, the authors rely on a quantitative content analysis of all "speeches and writings" by the six Soviet officials during 1970–72. They measure the ratio of "references" deemed "supportive" or "resistive" to Sadat's demand for weapons deliveries. A reference to advances in the Third World is considered supportive, while a reference to East–West détente is considered resistive.

One may question whether the ratio of references carries any significance. Any speech or article is not a concatenation of isolated references but a coherent text. A feature especially pronounced in Soviet political discourse was the tendency for speakers to communicate differing policy preferences not only by varying the number of references to agreed policies but also by varying the manner in which these references were combined (Ploss 1971, 120). For example, in each of two speeches given by the oligarchs Kosygin and Podgorny two days apart in October 1971, references to advances in the Third World considerably outnumber references to East–West détente. But Podgorny says that advances in the Third World will make détente feasible, while Kosygin says that détente is necessary for advances in the Third World. Because meaning can depend on overall organization of a speech, the ratio of references to one policy or another might vary without any change in the attitude expressed by the speaker.

Rather than relying on quantitative content analysis, the authors might examine Politburo members' explicit comments on the Arab–Israeli conflict. They would discover that the Politburo's specialist on Mideast issues during 1970–72 was Podgorny, whose role they (following Spechler 1986) overlook entirely. Although the authors score him as paying zero attention to the Mideast, in fact, 10 of his 70 speeches concern the Mideast almost exclusively; and he addressed it in other speeches. He commented on the Mideast much more than any other Soviet leader. In opposition to Sadat's demands for arms, Podgorny advocated a "political settlement" to be achieved not by direct U.S.–Soviet or Israeli–Arab negotiations but by building a diplomatic coalition that would isolate Israel and the United States and compel unilateral concessions.[1]

Until 9 December 1971, Brezhnev and Kosygin echoed Podgorny's calls for a political settlement. But at summits with Sadat in *February and April 1972*, from which Podgorny was excluded, Brezhnev and Kosygin agreed to communiqués that shifted Soviet declaratory policy in favor of Sadat's view that offensive war was a justifiable solution to the Arab–Israeli conflict (Glassman 1975, 94; Rubinstein 1977, 179; Porter 1984, 122, n. 19). Their speeches began to call for "liberation" of occupied Arab territories without repeating Podgorny's strictures against military means.[2] Podgorny, meanwhile, continued to advocate a political settlement in the Mideast; but he also lost his 1971 predominance in the public exposition of Middle East policy.[3]

Thus, the shift in Kosygin's declaratory stand on the Mideast conflict—the first link in the authors' causal chain—occurs at least five months before the event, Sadat's expulsion order, which the authors say causes his attitude to shift.[4]

A DOMESTIC BARGAINING MODEL

To explain the Soviet decision to arm Egypt, one might turn to a simple domestic bargaining model of foreign policy. The bargaining model posits that each national leader seeks to control policy on as many issues as possible. (It lets stand the question *why* leaders want control of policy—perhaps because they are ambitious, perhaps because they think their policy preferences are optimal for their state.) Each national leader is assumed to have two choices of action: (1) to insist on his or her own policy proposal and (2) accept some other policy proposal. If some leaders accept some other policy proposal than their own when one leader insists on his or her own, the "other" policy is assumed to be the insistent leader's own proposal; if all leaders accept some proposal other than their own, the policy is assumed to be some intermediate compromise. This set of assumptions produces a game matrix, shown in Figure 1 for the two-leader case.

If the assumption that leaders want to control policy is interpreted to mean that they would rather disagree than surrender policy to another leader's control but prefer a mutually acceptable policy to disagreement, this game is the familiar prisoner's dilemma. It is iterated over indefinitely many policy decisions over time. This particular prisoner's dilemma has the interesting feature of uncertainty about who plays last. For example, under the rules in 1972, Politburo members could die in office or fall ill and retire or the Central Committee could remove them from office. Consequently, this indefinitely iterated prisoner's dilemma is not subject to the usual logic of backward induction, and there are many possible equilibria (Tsebelis 1990). Among these equilibria is an "alternating sucker" pattern in which each leader accepts an insistent leader's proposal on one issue in return for obtaining acceptance of his or her insistence on some other issue (Hardin 1982; for a spatial formalization, see Austen-Smith and Banks 1990).

In order for policy to change in response to world events, the model further assumes the existence of an informal norm among national leaders. Any leader who proposes a policy must justify it by setting forth a public agenda of goals that the leader promises the policy will accomplish. According to the posited norm, information that the policy is not accomplish-

FIGURE 1

A Foreign Policy Bargaining Game

		Leader 2	
		Accept other proposal	Insist on own proposal
Leader 1	Accept other proposal	Mutually acceptable policy	Leader 2's policy
	Insist on own proposal	Leader 1's policy	Disagreement: no policy

ing this agenda eventually requires the proponent leader to stop insisting on the proposal.

HISTORICAL RECONSTRUCTION

This model can explain the Soviet reversal of policy toward Egypt. Podgorny disagreed with Brezhnev and Kosygin about both Mideast policy and East–West détente. They wanted to negotiate with the United States on both issues. He wanted to negotiate only with friendly regional states: the Arabs in the Mideast, and France, Italy, and the small states in Europe. During 1970, these policy preferences imposed no particular choice on this subset of Soviet leaders, since Podgorny's desire for separate negotiations with West European countries could be accommodated in a general program of exploring détente through bilateral diplomacy with a variety of Western countries. Beginning in early 1971, Israeli recalcitrance and Henry Kissinger's duplicity combined to frustrate Brezhnev and Kosygin's efforts for superpower cooperation in sponsoring a negotiated settlement in the Mideast (Breslauer 1983), while the European NATO members' unwillingness to negotiate security issues separately from the United States frustrated Podgorny's program in Europe. Consequently, both Podgorny and the Brezhnev–Kosygin pair could invoke the informal norm; a compromise would combine Podgorny's policy in the Mideast with Brezhnev and Kosygin's policy on East–West issues. (The other three officials examined by the authors—Grechko, Suslov, and Ponomarev—were inactive on the Mideast; and Brezhnev and Kosygin used concessions on other issues to buy off their opposition to détente.)[5]

During 1971, Podgorny's speeches advocated his political solution to the Israeli–Egyptian dispute. Sadat, having declared 1971 the "year of decision," immediately complained about the gradualist aspects of Soviet policy but was willing in the short run to accept proposals for efforts through the United Nations to achieve an Israeli withdrawal from Sinai. However, as 1971 wore on without results, Sadat began to argue that Israeli rejection of the UN initiatives justified his demands that the Soviets provide offensive arms for his preferred military solution. He also sought a settlement through direct bilateral contacts with Washington (Golan 1990, 76–79). His spokesman Mohammed Haykal (1975, 1978) accused the Soviets of perpetuating a condition of "no war, no peace" for their own benefit.

By the end of 1971, Sadat's arguments, his contacts with Washington, and Haykal's accusations constituted information that Podgorny's coalition for a gradualist policy was falling apart. In fact, in a speech for the visiting Sadat in October 1971, Podgorny tried to rebut such concerns. Consequently, at this stage Brezhnev and Kosygin could invoke the informal norm again. They would prefer to return to a policy of negotiating a Middle East settlement in tandem with the United States. However, Kissinger's unwillingness to enter serious negotiations on the Mideast, combined with his efforts to lure Sadat away from the Soviet alliance, made their preferred policy impractical. As Kissinger himself writes, "[Foreign Minister] Gromyko was experienced enough to know what I was doing" (quoted in George 1983). Consequently, Kosygin and Brezhnev proposed a policy of maintaining the Egyptian alliance (and frustrating Kissinger's plans in the Mideast) by conceding to Sadat that Egypt had the legitimate right to undertake war against Israel.

At the same time, Brezhnev and Kosygin recognized that an Egyptian attack on the Sinai would seriously interfere with their policy of U.S.–Soviet détente. To manage this tension within their policy, they took advantage of Egypt's known lack of hard currency by insisting that Egypt pay cash for any new arms deliveries (Central Asian Research Centre vol. 1, p. 208 and vol. 2, pp. 93, 246). This demand prevented arms deliveries during 1972. Resorting to the only leverage he could imagine, Sadat ordered the expulsion of the Soviet military personnel in July 1972 (Sadat 1977). His move did not affect the Politburo bargaining; for he did not withdraw from the Soviet alliance, and keeping troops in Egypt was not part of Brezhnev and Kosygin's stated agenda. They willingly "overcomplied" with the expulsion order, withdrawing more troops and weapons than Sadat had asked (Golan 1990, 79; Roi 1975).

In January 1973, however, Saudi Arabia suddenly granted Egypt five hundred million dollars for the purchase of Soviet arms (Golan 1990, 84; Rubinstein 1977, 242). Brezhnev and Kosygin did not foresee this development; for their image of Arab countries distinguished "progressives" like Egypt from "reactionaries" like Saudi Arabia, said to be tacitly allied with Israel. Within days an Egyptian delegation left for Moscow to buy arms. If, as per my assumption, Brezhnev and Kosygin's purpose was to control policy on as many issues as possible, they would have acceded to the Egyptian demand for arms deliveries because this move would have enabled their policy proposal to retain control of the Mideast issue. Otherwise, they would have to admit that their policy had failed to maintain the Egyptian alliance and agree to some policy sponsored by someone else. Podgorny held them to their bargain with Sadat because he expected the arms deliveries to interfere with the détente that he opposed.

In short, the Soviet leaders chose a policy of delivering arms to Egypt that later interfered with U.S.–Soviet détente because this suboptimal policy in the international game was optimal in a domestic bargaining game.

CONCLUSION

Both process models and domestic bargaining models offer general explanations why states often adopt self-frustrating foreign policies. Process models attribute these policies to deficiencies in processing information about new events. In domestic bargaining models, leaders may be uninformed about circumstances and surprised by events; but they adjust their strategies reliably to new information. According to the domestic bargaining model, states adopt self-frustrating policies whenever these policies win in the domestic game, because a win in the domestic game is prerequisite for any policy to advance to the international game. In the case of Soviet arms for Egypt, the process model's focus on the impact of salient international events leads the authors to attribute the change in the Soviet leaders' attitude toward Mideast policy to an event, Sadat's expulsion order, that *followed* the observed change by five months. A domestic bargaining model avoids this causal inversion.

Bargaining models and process models are alternatives, not complements, because their explanatory logics make incommensurable assumptions about decision makers' use of information. The domestic bargaining model has the advantage of being much more compact than the authors' very elaborate process model. At the same time, the bargaining model preserves all the desirable features of the process model. Individual-level variables (preferences, perceptions of the international situation, information) combine with group-level variables (bargaining, norms) and state-level variables (rules affecting choice of equilibrium, number and identities of bargainers) to produce policy choices that react to international events but are contingent in the sense that national leaders can choose any of a wide variety of possible equilibria to the game. While the explanation of Soviet policy is consistent with evidence of oligarchic decision making by the Politburo, the model is well known to be general to policy choice, domestic and foreign, in other states.

This last point deserves emphasis. The propensity for strategic interaction within collectivities to produce behaviors suboptimal for the collectivity in larger social and natural environments is familiar from virtually every subfield of political science. Failures of information processing are inherently plausible as explanations for suboptimal behaviors, but they are inordinately difficult to detect against the background of the general disarray introduced into policy by collective choice.

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RESPONSE

Anderson, in critiquing the process model we employ to understand one puzzling case of a self-defeating foreign policy, claims (1) that the indicators we use do not adequately assess aspects of the process model, (2) that we have engaged in a causal inversion, and (3) that the domestic bargaining model is more parsimonious and better fitted to explaining the Soviet shift in early 1973 to granting Egypt's request for offensive arms. We would like to rephrase Anderson's criticisms into a set of questions and compare and contrast the process and domestic bargaining models in light of these queries: (1) Do self-defeating foreign policies reflect compartmentalized or competing attitudes among the leadership? (2) How do we know when attitudes change? and (3) Is the domestic bargaining model *too* parsimonious?

COMPARTMENTALIZED OR CONFLICTING ATTITUDES?

The differences between Anderson's and our approaches to assessing leaders' commitment to their preferences are at the heart of a debate among scholars studying the Brezhnev regime and lie at the heart of the questions that arise around self-defeating foreign policies. Two schools of thought have developed to account for the Soviet government's simultaneous pursuit of essentially contradictory policy lines during the 1970s. One—like Anderson—sees a consensus among the Soviet leaders about their image of the Soviet Union as a global and imperial power that included within it the complementary elements of military growth, Third World activism, and détente (e.g., Glassman 1975; Head 1982; Rigby 1970). As Head has noted, the leadership practiced "separatism"—the Soviet Union had "the right to seek cooperation with the West in some matters, while challenging it in others" (1982, 41). Differences among Politburo members focused on how these various elements should be combined—how each might condition, or be conditioned by, the others—rather than on which should be the center of attention.

The other school—the one we have followed—proposes that the sharp differences in Soviet policy reflected similar differences among the leaders about the nature of Soviet objectives and appropriate methods of achieving them. Which objective should be the number one priority was at issue. Politburo members held different images of the Soviet role in international relations. Consequently, Soviet foreign policy could shift dramatically as the coalitions supporting different objectives aligned themselves in different ways. The data from studies by Spechler (1986) and Stewart, Warhola, and Blough (1984) persuaded us of the appropriateness of following the conflict theory approach and developing a ratio measure of commitment to preference (see also Dawisha 1979 and Kass 1978).

Spechler (1986) identifies two dominant images toward the United States among Politburo members during the years 1967–73. These images, she argues, had implications for the Arab–Israeli conflict and the decision to provide Egypt with offensive, strategic weapons in 1973. She labels the two the "cooperative" and "antagonistic" images. Members with the cooperative image "were impressed by the fragility of détente. . . . They did not want events in the Middle East (or in any other region) to cause or deepen mistrust and hostility between the U.S. and the U.S.S.R." (Spechler, 1986, 448). Those with the antagonistic view argued, "The U.S. must be actively countered and restrained, both militarily and politically. Highest priority must therefore be given to expansion of Soviet military power and political influence on a global scale. The Soviet Union must acquire friends and military facilities and maintain loyal allies wherever these would help to combat, contain, or undermine American power" (p. 450). In Spechler's study of the Soviet decision to give arms to Egypt, Politburo members held one or the other of these images, *not* both. In effect, the images were inverse, not conditional.

In support of Spechler's argument for the conflict between these two images in Soviet foreign policy during this time period, we note the content analysis of themes surrounding East–West relations done by Stewart, Warhola, and Blough (1984). References to détente and Soviet military power were the most prevalent in the 1970s when compared to all other themes (43% and 28% respectively), reflecting the importance of these two issues in Soviet foreign policy discourse. An R-factor analysis of all the themes across Politburo members indicated that these two foci were in competition with one another. Factor scores for individual Politburo members showed that the spokesmen for détente were not the spokesmen for increased military power, and vice versa.

The emphasis on either compartmentalized or conflicting attitudes has led Anderson and us to employ different techniques to measure commitment to preference. Anderson has used a more qualitative content analysis procedure in developing leaders' positions. He is as interested in noting when phrases describing policies and approaches change as in keeping track of how often a particular theme is repeated. He focuses on how the Politburo members link the various streams of Soviet foreign policy to deal with what appear on the surface as contradictory ideas. One result of this focus is his interest in conditional policies. We, on the other hand, have used a more quantitative content-analytic approach that looks for how much Politburo members emphasized certain themes relative to other themes. We have paid particular attention to which leaders have focused on which issues to facilitate our understanding of the positions each is likely to advocate in a decision-making process where there are basic disagreements over objectives.

HOW DO WE KNOW WHEN ATTITUDES CHANGE?

One of the important concerns of both Anderson and us is to determine when Brezhnev and Kosygin began to be influenced by the environment to change their attitudes regarding supplying Egypt with offensive, strategic weapons. Because these two leaders are classified as cuetakers by our process model, we are interested in how contextual information shaped their positions. What environmental cues triggered a change has implications not only for the positions of these two leaders but for what happened in the group process as the Politburo worked to solve the problem. Ascertaining such environmental impacts is important for Anderson, as well, in determining when Brezhnev and Kosygin's views began to diverge from Podgorny's and, thus, when the bargaining equilibrium started to shift from Podgorny. Anderson argues that Brezhnev and Kosygin changed their positions in December 1971, or at least by February 1972, when they stopped advocating a political settlement. We focus on Sadat's two dramatic steps in the summer and fall of 1972 (expelling Soviet military personnel and returning "hat in hand" to seek Soviet support when overtures to the West were rebuffed) as catalytic agents for shaping Brezhnev and Kosygin's positions. In effect, Anderson believes that the change occurred from five to seven months earlier.

For Anderson, it is a change in the expressed attitude that took place by February 1972. Brezhnev and Kosygin stopped advocating a political settlement of the Arab–Israeli dispute and began shifting toward Sadat's position, indicating that means other than diplomacy could be used in dealing with Israel. Anderson refers to a shift in "Soviet declaratory policy." He argues that Brezhnev and Kosygin could shift their rhetoric and not interfere with their policy of détente with the West because they added the caveat that Egypt would have to pay hard currency for any new arms deliveries. They knew Egypt did not have sufficient hard currency to buy the weapons. Their bluff was called in January 1973 when Saudi Arabia provided Egypt with the hard currency for Soviet weapons; and the Soviets sold offensive, strategic arms to Egypt.

As Anderson describes the scenario, his choice of words for Brezhnev and Kosygin's positions seems quite appropriate—a change in Soviet "declaratory policy." The two Soviet leaders changed their public posture, but did they change their attitude before their bluff is called? Indeed, this change in public posture without a coterminous change in Soviet foreign policy behavior led to Sadat's frustration and his expulsion of Soviet military personnel in July 1972. The Soviet leadership appeared hypocritical—saying one thing and doing another. In a description of the communiqués issued from the series of meetings between Soviet and Egyptian leaders between February 1972 and July 1972, Rubinstein notes the increased tension and disagreement occurring as Sadat heard Brezhnev and Kosygin's words encouraging him to use other means than a political settlement to deal with the Israelis but saw no action in providing him with the military support he needed (1977, app. 4). As Rubinstein notes for the meeting on 27–29 April 1972, "Moscow expresses 'full support' for Arab efforts, though, 'the sides found it necessary to study again in a spirit of fraternal cooperation measures' for increasing the military potential of Egypt" (p. 362). At the meeting on 13–14 July 1972—just three days before Sadat's expulsion of Soviet military personnel, Rubinstein observes, "the brevity of Sidqi's visit suggested tension and disagreement" and "the mention of economic cooperation serves to highlight the absence of any mention of military cooperation or of Soviet efforts to increase Egypt's defense capability" (p. 363). In effect, as Anderson has recorded the events, the Soviet leaders' attitudes may never have changed. By providing hard currency to Egypt, the Saudis helped Sadat to call the Soviet bluff. As a result, Brezhnev and Kosygin were forced by the strategies they had used to maintain their position to act contrary to their preferences.

In our application of the process model to this case, we present a table with commitment-to-preference ratios for the Soviet leaders whose positions counted in this decision for each of five quarters beginning with the first quarter of 1972 (Stewart, Hermann, and Hermann 1989, Table 3). *Only* in the first quarter of 1973 are the commitment-to-preference ratios for three of the leaders more than one standard deviation above the mean for the Politburo and in the direction of supporting sending offensive weapons to Egypt (over .50). Two of them (Kosygin and Suslov) are leaders in the oligarchy at this point in time; that is, by the first quarter of 1973—*after* Sadat's two dramatic moves—half of the oligarchs whose positions counted have become advocates for supplying Egypt with such weapons. Throughout the period, Grechko, the defense minister, has scores one standard deviation above the mean—the lone advocate for supporting Egypt militarily. Interestingly, Kosygin's commitment-to-preference ratio increases dramatically in the third quarter of 1972 during the period when Sadat expelled Soviet military personnel (going over .50) and again when the decision to provide weapons is made. He appears to have been affected by what was happening to Soviet influence in the Middle East as a result of Sadat's behavior.

Brezhnev's scores remain an anomaly, staying close to the mean of the Politburo across the five quarters. We hypothesize that as a cuetaker Brezhnev may have been more interested in what was going on in the Politburo than what was happening in the international environment. He retained his position based on support among his colleagues in this body. Thus, when there was disagreement among members of the Politburo, we would argue, Brezhnev made a conscious effort to determine where everyone stood, wanting to reflect the consensus among the members and not be too different until the position of the group was fairly well defined. There is some support for this proposition in Valenta's (1979) study of the Politburo's decision to intervene in Czechoslovakia in 1968. Brezhnev sat on the fence between the arguing factions until a definite consensus began to emerge.

Thus, our data suggests that the change in Politburo members' attitudes, particularly Kosygin's and Suslov's, occurred in the early part of 1973 after Sadat's moves away from, and then back toward, the Soviet Union. The data also indicate that Kosygin's reaction to the expulsion of Soviet military personnel may have helped to begin a reevaluation of the situation among Politburo members. In any case, there appears to have been a growing consensus in the first quarter of 1973 to supply Egypt with offensive, strategic weapons. The knowledge of a change in the consensus among Politburo members probably led Brezhnev to acquiesce and agree to providing Egypt with the weapons Sadat wanted.

IS THE DOMESTIC BARGAINING MODEL TOO PARSIMONIOUS?

The process model asks three questions: (1) Whose positions count in making a decision on a particular issue ? (2) What are their positions on the issue under consideration? and (3) How are disagreements handled? As a consequence of these three questions, the process model examines decision making using three levels of analysis: the individual, the group, and the state. In some interesting ways, the process model is a more inclusive model that provides the input information required by the domestic bargaining model. For example, Anderson does not question the part of the process model that indicates whose positions count—which Politburo members had influence and had to be included or consulted before a decision was made. He assumes this part of our model, as he concludes that the Brezhnev regime during the period 1970–72 was an oligarchy. The domestic bargaining model does not provide a mechanism for deciding whom to focus on as the bargainers in a decision-making body. They must be stipulated by the researcher. The process model includes a way to determine whose positions count.

Data from the process model are also useful in assessing which of the two positions the bargainers are likely to take in the domestic bargaining model (accept another proposal or insist on one's own proposal; see Figure 1). The advocate in the process model is equivalent to the bargainer who insists on his own proposal; the cuetaker is equivalent to the bargainer who is willing to accept another proposal. In the process model, using personality and organizational information, we are able to identify different types of advocates and what cues are likely to be valued by the cuetakers. Thus, there are advocates who push their positions *only* when the timing is right or their position is congruent with that of their organization, as well as advocates who persistently push their positions or their organization's agenda *regardless* of the situation. Cuetakers likewise differ on where they look for cues on appropriate behavior. Some focus on the particular situation, others on their patrons or clients, and still others on their organizations. Again, the process model provides the means for determining the positions needed—but not established—by the bargaining model.

Assuming Anderson's description of Podgorny to be correct, we probably have in this leader an advocate who was interested in pushing his position, perceiving only the information from the environment that supports what he believed was right. There was little flexibility when he was strongly committed to something. As we have already noted, the data in our study indicate that both Brezhnev and Kosygin were cuetakers who, though focusing on different kinds of cues, used situation-specific data in making up their minds, ever taking stock of context factors before assuming a position and cautious in jumping to conclusions without knowing what was going on around them at a particular point in time. In terms of the domestic bargaining model, then, using information from the process model, we would expect Podgorny to have insisted on his proposal in a competitive and aggressive manner and Brezhnev and Kosygin to have been willing to accept another's proposal and, most likely, seek a mutually acceptable policy. The data Brezhnev and Kosygin would use in seeking a mutually acceptable policy, however, would be different. Brezhnev would want to know what his colleagues were thinking; Kosygin would be interested in what was happening in the international environment regarding the particular issue under debate.

An examination of Anderson's historical reconstruction suggests the accuracy of these profiles. Throughout his description of what happened in the bargaining process during 1971–73, Anderson notes how events were driving Brezhnev and Kosygin while Podgorny continued to hold the position he began with.[6] Podgorny held to his original point of view, while Brezhnev and Kosygin responded to what was going on in the Politburo, as well as to Israeli recalcitrance, Sadat's growing frustration, and Saudi grants of hard currency to Egypt, in fashioning a policy that tried to respond to the situation that they perceived was facing them at the time.

The domestic bargaining model and our process model are most similar in stipulating rules for how Politburo members resolve differences and disagreements among themselves. In both, the models are focused on the dynamics in the group as members try to reach a decision. When those who count in the decision-making process disagree and come into conflict, how they go about resolving this conflict is at issue. Interestingly, the outcomes of the two models mirror one another. In the process model, "adopt oligarch's position" is similar to the case in the domestic bargaining model where Leader 1's or Leader 2's policies are selected. In the process model, deadlock can occur just as in the domestic bargaining model. And compromise is possible in the process model, matching "mutually acceptable policy" in the domestic bargaining model.

In actuality, the process model helps to spell out some of the activities in the group that underlie the domestic bargaining model. Whereas the domestic bargaining model searches for equilibria and builds on the rules of nested games, the process model arises out of the group dynamics and organization literatures where the emphases are, among others, on understanding majority–minority influences, building cohesion, achieving compromise and consensus, preventing deadlock, and developing decision norms. The process model offers ways to explain why certain equilibria are possible, given the configuration of roles and positions in a decision unit.

CONCLUSION

If our aim is to understand and explain how foreign policy decisions are made, it seems important to use a variety of models and methods to study self-defeating policies such as this Soviet decision. Only if we engage in these endeavors, can we begin to see how the models might interrelate, might explain different aspects of the decision process, might be relevant under different conditions, or might hold in different contexts. Moreover, only by taking different perspectives on the problem and making different assumptions can we gain a multifaceted view of leaders' preferences, positions, and roles. The tendency to see models and methods as always in competition with one another with only one supposed to win and supersede the others does not facilitate such an exploration. We may learn more by comparing and contrasting the models, as we have done in this point–counterpoint analysis, than by focusing on one model to the exclusion of all others.

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Notes

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1. Factual assertions concerning the contents of Soviet leaders' speeches during 1970–72 are taken from my dissertation (Anderson 1989).

2. For Kosygin, see *Pravda*, 12 February, 10 April, 4 July, and 3 and 17 October 1972. For Brezhnev, see *Pravda*, 9 December 1971 and 19 March, 6 and 28 June, and 14 November 1972. Kosygin's speech published on 17 October 1972 coupled the call for liberation with a call for a political settlement as an alternative if possible; his speech on 25 October defined a political settlement as equal to an Israeli withdrawal from all Arab territory. Brezhnev's speech on 22 December adopted this same position.

3. For Podgorny's continuing advocacy of a political settlement, see *Pravda*, 13 October and 8, 9, and 14 December 1971 and 13 April, 7 July, and 15 September 1972. Note that Podgorny's three references to a political settlement during December 1971 coincide with Brezhnev's abandonment of this phrase.

4. The literature disagrees on the importance of the expulsion to the Soviet decision. Spechler 1986 and Porter 1984 concur with the authors; Dawisha 1981, Freedman 1978, Glassman 1975, Golan 1977 and 1990, and Roi 1974 and 1975 all attach much less importance to Sadat's action.

5. Grechko possibly preferred to grant Sadat's requests for arms, but the Politburo had an arrangement with the high command that the generals could have autonomy in military policy if they accepted the Politburo's right to decide larger foreign policy issues (Colton 1979; Colton and Gustafson 1990; Rice 1987). Suslov and Ponomarev concentrated on relations with world communist parties. Because communist parties were inactive in Egypt and Syria, the Mideast issue played little role in world communist politics.

6. See n. 3

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FORTHCOMING IN DECEMBER, 1992

The following articles, research notes, and controversies have been tentatively scheduled for publication in the December, 1992 issue:

Paul Allen Beck, Lawrence Baum, Aage Clausen, and Charles E. Smith, Jr. "Patterns and Sources of Ticket-Splitting in Subpresidential Voting."

Christopher Colmo. "Theory and Practice: Alfarabi's *Plato* Revisited."

Ada W. Finifter and Ellen Mickiewicz. "Redefining the Political System of the USSR: Mass Support for Political Change."

Thomas Hartley and Bruce Russett. "Public Opinion and the Common Defense: Who Governs Military Spending in the United States?"

Ken Kollman, John H. Miller, and Scott E. Page. "Adaptive Parties in Spatial Elections."

Eileen L. McDonagh. "Representative Democracy and State Building in the Progressive Era."

Richard D. McKelvey and Raymond Riezman. "Seniority in Legislatures."

Cary J. Nederman, "Freedom, Community, and Function: Communitarian Lessons of Medieval Political Theory."

Charles S. Taber. "POLI: An Expert System Model of U.S. Foreign Policy Belief Systems."

Paul V. Warwick. "Economic Trends and Government Survival in West European Parliamentary Democracies."

L. Marvin Overby, Beth M. Henschen, Julie Strauss, and Michael H. Walsh. "Courting Constituents?: An Analysis of the Senate Confirmation Vote on Justice Clarence Thomas." A research note.

Motoshi Suzuki. "Political Business Cycles in the Public Mind." A research note.

John Bendix; Bertell Ollman; Bartholomew H. Sparrow; Timothy P. Mitchell. "Going Beyond the State." A controversy.

ERRATUM

In the Abstract of the controversy "Question-Wording and Macropartisanship" in the June 1992 issue, p. 475, lines 6–7, the commenting authors should have been identified as MacKuen, Erikson and Stimpson.

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POLITICAL THEORY

We the People: Foundations. By Bruce Ackerman. Cambridge: Harvard University Press, 1991. 369p. $24.95.

This book of essays is an outline of, and introduction to, a larger work that will eventually include two more volumes. The guiding and very persuasively argued thesis set out here is that the United States is a "dual democracy" in which "We the People" assert ourselves occasionally in acts of revolutionary constitutional politics, while mostly it is governed by the normal politics of its elected representatives. The Supreme Court acts as the agent of "the People" during the long calm periods by preserving the Constitution against the "erosion" of normal politics (p. 87). As such, it is an eminently democratic institution. This argument is not entirely new. I first heard a version of it in the early 1950s in the lectures of Robert McCloskey, who used to say that the American people wanted both change and continuity and that judicial review guaranteed the latter. It was even suggested that this was a realization of Rousseauian democracy in which the will of all, or at least of the majority, prevailed most of the time, to be checked when necessary by the higher general will of the people.

Ackerman would probably reject any such introduction of European philosophy into the interpretation of American constitutional history. He, in fact, begins with an outburst of truly Jacksonian populism, denouncing just such theoretical European distortions of the native experience, and especially of the majesty of "We the People." In truth, his own version owes more than a little to Hegel. In what he calls his "story," institutions have a historical logic and dialectic of their own, quite apart from the preferences and beliefs of the individual agents who at any moment administer them. This is, moreover, especially true of the courts, because Ackerman is wholly convinced that theirs is an autonomous history within America's culture and that bench and bar form a discrete community that has historically followed its own path (p. 39). Unfortunately, he merely asserts this conventional common lawyer's ideology without offering any arguments in its favor—a great pity, because it simply imposes an excessively narrow framework upon his otherwise ambitious project. As it is, Ackerman is only "gesturing" toward the social sciences and history (p. 60). He is certainly not writing for political scientists, since in the second half of the book he openly addresses readers who have never read, much less thought about, *The Federalist Papers* (p. 200).

In spite of these oddities and of far too many rhetorical flourishes, the first part of this book is extraordinarily interesting, intelligent, and lucid; and it is sure to raise the intellectual level of the whole field of constitutional theory. In Ackerman's new "story," American constitutional history is neither a seamless web nor a succession of ups and downs. Instead, it has been run on two tracks, one revolutionary and the other adaptive. There have, he argues, been three acts of radical adjustments between generations: the ratification of the great document in 1787, the three Reconstruction Amendments, and the New Deal. The latter differs from the first two in that it was not preceded by violent warfare and that it amended the Constitution not by following the procedures of Article 5, but wholly by judicial interpretation, after the "switch in time." It was a genuine revolution, nevertheless, because it created a new activist state and, with its acceptance, a renewed and aggressive judicial emphasis on the Bill of Rights. Jacksonian democracy, on the other hand, does not qualify as a revolution, because it was "not sweeping enough", failed to promote the nationalization of politics, and its tribunal presidency did not last (pp. 77–80). Judicial doctrines did not really shift, either. One might, of course, consider that the adoption of universal white manhood suffrage was a major democratic achievement, as was the recasting of democratic ideology, both of which set America forever apart from Europe. This was, however, played out in the states, where most of the political action was, in any case—which is why it can have no part in Ackerman's exclusively national "story."

Beginning, as one must, with the Marshall Court, Ackerman shows how it preserved the essence of the new Constitution against the localizing tendencies of normal politics. It sustained the centralizing political thrust of the Constitution, as well as its guarantees of rights, in the memorable *McCulloch* and *Dartmouth* decisions respectively. Above all, Marshall set the Court its permanent task—the protection of the revolutionary new governmental order against the demands of regular politics. This was to be the function of the Court when it was confronted by the three Reconstruction Amendments, as well. While, at first, it treated the new Constitution as applying only to African-American ex-slaves, it soon extended "the equal protection of the laws" to cover the contractual and property rights of white citizens, as well. The market was its paradigm, and for that time and place it was a genuine work of integration as the new rights were absorbed into the Federalist doctrines of the first era of interpretation. For the Court understood that the people had acted to create national citizenship and an energized laissez-faire economy, free of the impediments that slavery, sectionalism, and their attendant political inhibitions had imposed on the pre–Civil War order. In that view, *Lochner* was quite appropriate in 1905, though it was no longer acceptable in 1932. With the New Deal, the Court learned to accept the regulatory state and the finally institutionalized "energetic" presidency that Hamilton and Jackson had promoted in vain. Above all, beginning with Chief Justice Stone's celebrated footnote in *Carolene Products*, the Court restored the Bill of Rights to primacy in order to protect personal and minority rights within the now enormously expanded powers of the state. The break with its immediate past was thus not really complete, since the Court saw to it that the new state was still limited in its powers. Moreover, the Warren Court was no more "prophetic" than its predecessors (pp. 140–42). Even *Brown* was only a part of the New Deal's comprehensive state activity, extended to public education with due regard to the tradition of "the equal protection of the laws." It was not a return to the Marshall Court, as some claimed, but an adaptation of its doctrines to wholly new circumstances and a measured response to the more immediate past. This story has at least two important implications. First, it reveals the Court as the guardian of those few truly revolutionary amendments when "the People" act directly and not through their "stand-ins." Its does so ultimately by integrating these radical changes into accumulated interpretive practices. Second, Ackerman also neatly overcomes the old question so often put to liberals, "Why are personal rights more important than property rights?" If one accepts, as he does, that our rights are to be found only in the Constitution that "the People" have made and remade, then one simply recognizes three successive episodes in which rights were given new meanings and applied to new claimants. In this scenario, all rights have the same standing and source—and all undergo the same fate—as history imposes its limits on every successful revolution.

"We the People" are only the politically active citizenry in Ackerman's story. Americans are normally "private citizens" who become politically engaged only sporadically. He does not particularly regret their preference for civil society. The Constitution was made for such people, and it does not require Jacobin, virtuous, full-time citizens. An occasional Ralph Nader is quite enough (pp. 232–33). Nevertheless, Ackerman does think that the amending process should be made more like a referendum and offer a greater opportunity for presidential initiative. This would evidently make the mobilization of "the People" for revolutionary change easier and more frequent. Actually, Jefferson proposed something of the sort only to be soundly refuted by Madison in *Federalist* 49. Ackerman does not mention this, perhaps because he seems so eager to deny his own perfectly obvious Jeffersonian roots. He is none the worse for them; for though he is not as insouciant about bloodshed as Jefferson was (Who in our century could be?), he, too, advocates transforming popular action, in a modified version of Hannah Arendt's idea of a political revolution with some social issues added. Her definition, moreover, justifies Ackerman's applying the term *revolution* to his three transforming moments of constitutional history. It is an encouraging thought, as well, meant to inspire "the People" to assert their sovereignty again. The Supreme Court could presumably use a new amendment to divert it in time from its current course. In the meantime, we can look forward to the continuation of Bruce Ackerman's interpretation of our constitutional history and hope that he will expand his story to include people and events that lie beyond the confines of a hermetically sealed legal history.

Harvard University JUDITH N. SHKLAR

The Prevalence of Deceit. By F. G. Bailey. Ithaca: Cornell University Press, 1991. 143p. $24.95 cloth, $7.95 paper.

Positivism has been displaced, or so we assume. The program of making everything knowable through the supposedly impersonal norms and procedures of "science" has been radically questioned. The hope of constructing a "grand narrative," either intellectual or political, that will give us the ultimate truth and will lead us to freedom has been shattered in many ways. Reality, it seems, is a text subject to multiple interpretations and multiple uses. The implications of this for research and politics in our supposedly "postmodern" age are more than a little interesting.

There are few words more often used—and abused—in discussions of contemporary culture and politics than postmodernism. It is hard, in fact, to find an area of study where it has *not* had an impact. In general terms, the form postmodernism takes is that of the self-conscious, self-contradictory, self-undermining statement. It wants to provide a thoroughgoing "denaturalizing" critique, to "dedoxify" our cultural representations. All of our discourses are politically "uninnocent." They occur within a dynamic social context in which the existence of multiple sets of power relations are inevitable. Hence, discourse and politics, knowledge and power, seem to be part of an indissoluble couplet.

All of this is provocative. Yet while much of the postmodern project is insightful and serves as an important corrective to what even progressive scholars take for granted, there are still parts of postmodernism that should and do make one pause. Its capacity for overstatement, its own rhetorical flourishes, its use by all too many people to make often simple points that we sometimes already knew from other traditions—all of this detracts from some of its strengths.

Bailey avows parts of the deconstructionist program while offering substantive (and occasionally simply rhetorical) criticisms of it. Even when I was not always in full agreement with his reproofs, constructing responses to them was quite an enlightening enterprise.

Bailey himself is a reconstructed positivist, one who wants to inquire into the conditions in which we test "truth" in a world of contested values. Because of this, *The Prevalence of Deceit* is true to its title. Social life is organized around "basic lies" and smaller lies. Truth is a contest, it is not simply there. The language of truth and deceit has multiple uses. It provides the cement of social interaction and is malleable depending on its historical and cultural context. Truth and untruth are the very instruments of politics. Bailey's discussion of this is based on multiple examples, often taken from his own earlier anthropological fieldwork in India.

In the process, he offers comments on how the issue of truth and lying has been dealt with in previous theories, how we are to understand the process through which dominant groups gain hegemony, how negotiations over "the truth" involving differential power relations go on, the place of deceit in ethnographic research, and the status of truth claims in such research. The volume is really a wide, varying essay, more a "reflections on" than a detailed treatment of its subject. To prove his case that in political and cultural life, truth is "mostly illusory" and that the "life we share with others must include untruth" in a variety of forms (p. xxi), Bailey would have had to expand his examples and arguments considerably. While this gives some of its content a rather stipulative feeling, what he does present is nicely complemented by an engaging and often bitingly humorous style.

The volume is not without its conceptual and stylistic problems, however. There are times when, even given Bailey's elegance and wit, the reader is not at all certain where the book is going. Further, I was surprised that Bailey did not draw upon Murray Edelman's work on political language or James Scott's discussions of public "performances" that hide acts of resistance. This might have lent even more strength to some of his claims about relations between dominant and dominated groups.

Finally, Bailey at times does not always fully represent the more sophisticated arguments already made either defending or criticizing interpretive/hermeneutic anthropological theories and postmodern approaches. Although I, too, have strong reservations about aspects of interpretive and postmodern theories, especially their tacitly apolitical tendencies behind their rhetoric, these theories are not composed of simple arguments. They require detailed exposition of their *best* case; then one can set about doing a more powerful critique.

All this said, I do recommend *The Practice of Deceit.* Few books actually force us, by both their style and their content, to engage in a good debate. Bailey has offered one such book.

University of Wisconsin, Madison MICHAEL W. APPLE

Identity\Difference: Democratic Negotiations of Political Paradox. By William E. Connolly. Ithaca: Cornell University Press, 1991. 244p. $27.50.

In a letter to his brother Theo, Vincent Van Gogh cautioned, "We must not judge God from this world, it's just a study that didn't come off. It's only a master who can make such a blunder." The statement expresses awe at worldly existence without expressing resentment of its shortcomings. Yet both here and in his art Van Gogh conveys that for all its wonders, there is much pain in life and that we decidedly do not live in the best of all possible worlds. One might say that William Connolly's *Identity\Difference* offers a secular and political translation of these sentiments.

For inspiration, Connolly primarily looks to Friedrich Nietzsche, who did with prose what Van Gogh did with paint. The purpose of Connolly's book is to investigate the psychology of resentment, so trenchantly described by Nietzsche, that stems from our unwillingness to accept the contingent and ungrounded aspect of the human condition. Connolly also looks to Michel Foucault, who offers historical analyses that illustrate the means by which resentment finds its social and political expression in disciplinary regimes, normalizing procedures, and processes of exclusion. Following Nietzsche, Foucault has called these investigations "genealogies" because they illustrate the pedigree of the identities we take for granted. As an academic strategy, Connolly strongly advocates for them. Departing from Nietzsche, Foucault promotes an agonistic and *democratic* politics that seeks to avoid, or at least severely limit, the imposition of identity and the penalizing or pathologizing of difference. As a political program, Connolly strongly advocates for this. Such democratic practice, Connolly admits, will not eliminate but will at least "expose and redress the politics of resentment" (p. 192).

Identity\Difference is, to my mind, the best-written of Connolly's books. It is stylistically original and captivating. Formally, its major problem is its repetitiveness. The reader is frequently reintroduced to the book's central theses, which get dressed up in different attire for different events but generally fail to make new impressions. The gist of his 34-page "Letter to Augustine," for instance, already elaborated in earlier chapters, could have been nicely conveyed by Nietzsche in a few aphorisms. Notwithstanding such formal criticism, I consider *Identity\Difference* to be the best work to date that theorizes politically about the effects of existential resentment. In the spirit of Connolly's desire agonistically to "negotiate" with his readers about the political paradoxes this theorizing begets, I shall illustrate Connolly's impressive achievement by questioning a few of his tactics.

Connolly highlights the effects of resentment by focusing on the "problem of evil." He examines Saint Augustine and the "discoverers" of America as examples of those who persecute or conquer others because they are incapable of celebrating difference. Evil appears to exist, for the world is imperfect. Unwilling to ascribe the root of evil to their God or to themselves, these powerful men indignantly thrust its burden on those different from them. The Manicheans, Pelagians, and Donatists suffered for their otherness at the hands of Augustine. Similarly, the native dwellers of America suffered at the hands of Columbus and Cortez, who attempted to conquer them, and the priest Sepulveda, intent on their conversion to Christianity. Connolly extends this analysis to modern "territorial democracy," wherein national solidarity is bolstered through the creation of external threats. As the old quip goes, a nation is a group of people with nothing in common except an enemy. A democratic politics that questions national boundaries, celebrates difference, and struggles against existential resentment, Connolly suggests, would offer us an alternative to the age-old alleviation of our existential insecurities by the facile imposition of evil identities on other individuals and nations. The goal is certainly admirable. Connolly's means of promoting it, I would suggest, require rethinking on at least two counts. First, despite Connolly's globalist aspirations, his own analyses, resources, and discussion remain fundamentally Western (not to say, American) in orientation and scope. Despite his call for us to escape the territorial state, Connolly's own work seldom leaves its own shores to encounter the other. Second, Connolly challenges us to eschew the attempt to secure the purity of our own identities by the simplistic and dangerous imposition of evil identities on others. Yet his own attack on the critics of postmodernism and other assorted opponents often fails to escape this type of behavior. In Connolly's hands, "[Kenneth] Waltz becomes the Sepulveda of his time" (p. 51); "[Robert] Keohane is the Columbus of modernity discovering the postmodern" (p. 55); and we find "[Charles] Taylor and [Jean-Paul] Sartre . . . embodying modernizations, respectively, of Augustinianism and of a heresy (Pelagianism) it spawned" (p. 113). These characterizations are stimulating. But they do not exemplify the ambiguous nature of identity advocated by Connolly.

Identity\Difference "reverses the emphasis" of his previous works, Connolly claims, by focusing more on the effects of resentment than the "social injustices" that may foster it (p. 34). But, and this is my third substantive criticism, the reader of *Identity\Difference* is given *no* reason, theoretical or empirical, to believe that social injustices, however defined, foster existential resentment. We are offered only the weak claim that "general access to economic, educational, and cultural opportunities *seems* to be one of the preconditions of more widespread engagement with the contingency of identity" (pp. 211–12; emphasis mine). In his magisterial investigation of the political effects of existential resentment, Connolly neglects to examine its social causes and therefore leaves us with no basis for understanding the social preconditions required for agonal democracy.

We are introduced to Connolly's text by an epigraph of Foucault's that celebrates curiosity as "a lack of respect for the traditional hierarchies of what is important and fundamental." It seems a fitting orientation for Connolly's postmodern project. Yet Connolly fails to notice that Heidegger (who, with Nietzsche and Foucault, forms the tripod supporting Connolly's endeavors) had a different view of curiosity. Heidegger often spoke of curiosity as an attempt to escape thinking about the fundamental ground of existence by way of a constant and eventually jaded search for novelty and the stimulus of innovation. As such, curiosity feeds the modern technological attempt to escape the limitations of our world and the burden of our being through the continuous fabrication of new things. My final criticism is that Connolly generally ignores the threat posed to sustainable democracy by technological ways of life and thought, as well as the potential charge that postmodernism often reflects just this attempt to flee the human condition and the hard choices of politics by means of literary fabrication coupled with a flippant iconoclasm and rootlessness.

Connolly is an unapologetic postmodernist. He has no pretensions that the world we inhabit and the lives we live are metaphysically grounded, ideally constructed, or even rationally coherent. For the most part life is a mystery whose tangles only further enmesh us the more we struggle to undo them. Nearly two-and-a-half millennia ago, Socrates seemed to say the same thing. He claimed to know only that he knew nothing. But then Plato, in Book III of the *Republic* and with political purpose in mind, has Socrates tell a lie; the truth of life's mystery does not allow for an orderly politics. So Socrates spins the myth of the metals, encouraging the belief that the gods have mixed gold, silver, iron, or bronze in people's veins at birth so that their endowments and destinies are predetermined. Unambiguous identities are established. Political order is achieved. Difference is tolerated only to the extent that it plays its prefigured role within the austere and restrictive structures of the city.

Identity\Difference is a courageous attempt to sketch a city in speech that rejects the need for noble lies. Like Plato, Connolly is necessarily ironic. That is, he is telling stories that make no claim to transcendent truth. Unlike Plato, Connolly makes no pretense that the stories he tells are noble ones. That is, he eschews that heroism investing Plato and Nietzsche (Foucault is explicitly invoked as an antidote to Nietzschean heroism on p. 10) that has one striving to raise one's program above all possible contenders. There is no attempt by Connolly to justify his theory as the means by which the polity at large, or even one's own soul, *must* be grounded. Thus, Connolly rejects any form of "command ethics" and opts for an "ethic of cultivation" that promotes, but does not ground, a caring for difference. He inserts his into the tumult of voices seeking our academic attention and political cooperation in the late modern world, not to "provide a single, exclusive interpretation but to place another candidate onto the field of discussion" (p. 171). Connolly's only firm conviction is of the partial (i.e., neither impartial nor totalistic) nature of the picture he paints. In short, Connolly's irony distinguishes him from the modernist, who "perpetually asserts . . . *as if* each time were at once the first and the last time it will be needed to refute the opponent" (p. 59). What separates Connolly from the ancients, and from Nietzsche, is his rejection of the need for nobility, which, as Nietzsche knew, is the oldest collective identity invoked by some that they might impose yet a different identity upon others.

University of Florida LESLIE PAUL THIELE

The Noble Savage: Jean-Jacques Rousseau, 1754–1762. By Maurice Cranston. Chicago: University of Chicago Press, 1991. 399p. $32.50.

The admiring readers of the previous volume of this biography (*Jean-Jacques Rousseau: The Early Life and Work, 1712–1754* (now available in paperback) have looked forward to this second volume; and, with confirmed enthusiasm, will be looking forward to the third. This volume begins with the *Discourse on Inequality* on the point of publication. It covers the years during which Rousseau was writing and getting into print *Julie*, the most successful novel of the eighteenth century; *Emile*, a hardly less considerable founding contribution to the philosophy of education; and *The Social Contract*, which would have sufficed by itself to make his name immortal. The volume ends with *The Social Contract* banned in France, and with Rousseau fleeing France, under indictment for provocation to recrudescent Catholicism offered in *Emile*, just banned, recalled, shredded, and burned.

He was leaving behind the best (and best-placed) friends that any original and versatile genius could have hoped for—the Marechal and Marechale de Luxembourg, whose patience, affection, magnanimity, and kindness for Rousseau resound through this book down the centuries. They were not the only kind friends that Rousseau had in these years nor the only ones who had to exercise patience to keep friendship up. Yet, touchy and often suspicious as he was, he was not at this time incapable of revising away his suspicions; and the touchiness reflected, in part, physical suffering and in part, a continual and, by Cranston's account, reasonably successful effort to maintain his exemplary independence. We find him again and again making difficulties about accepting gifts from any of his rich and noble friends and treating the Luxembourgs (grand and grandly helpful though they were) as friends, rather than patrons, all the while denouncing established society with undiminished vigor. Where affectionate relations did end, it was in one instance disappointment in love (for Sophie d'Houdetot, who fully returned his love for a time and long continued friendly and kind even after she had to break off) and in another a quarrel with Diderot after he had shabbily betrayed Rousseau to Sophie's established life companion.

Are these details of any importance to understanding Rousseau's thought? (The people just mentioned are only a few in the long procession that Cranston brings before us, keeping the whole cast of characters under such control that the information about them does not for a moment impede the forward motion of the narrative.) Cranston's synopses of Rousseau's works as they come up are excellent; but in them, the book gives us nothing new except grace in expression, polish, and economy. The story is engaging as the story of an interesting eighteenth-century life; but it might be engaging without having anything of importance to imply about the content of the hero's thought.

That is not the case here. There is something to learn about the depth of Rousseau's convictions from the aims and character of his attempt to set an example for his age. There are points to gather, from his correspondence with friends (more often than not, pastors) in Geneva, about his complex and discriminating attitude toward that city, its government, and its culture. It is of considerable importance to students of his work to observe his growing intellectual distance from the circle of the Encyclopedists, where he first gained intellectual credit. Cranston quotes liberally from Rousseau's letters in all these connections and thus usefully supplements the body of Rousseau's writings standardly read.

Yet the most important implications of the story told here are to be found in the sympathy and respect for Rousseau that its details tend to elicit. He was painfully and incurably ill with a defective urethra and with stone, yet he not only kept at work but enjoyed many happy moments and gave happiness to many others. He continued to be mixed up about sex, but he hardly fell outside our own normal range of confusion. He was not wildly irrational, to be looked to more for preromantic effusions than for orderly thinking. On the contrary, we see him carrying on a pointed and penetrating correspondence with his publishers. We see him judiciously discarding works that did not meet the standards that he had set for greatness, namely, *Les Solitaires*, a sequel to *Emile* in which both Emile and Sophie belie their promise and succumb to the corrupt temptations of the metropolis, and a sequel to *Julie* in which Milord Edonard disports himself at some length with women in Italy. The persecutions of which he became a target were under way, but we see Rousseau for the time being making no more of them than was warranted. He had given Voltaire provocation, and one can see that their ideas were irreconcilable; but it is Voltaire, not Rousseau, who displays malice and vindictiveness. We see Rousseau, in the midst of writing his greatest works, sincere, balanced, and clear-headed; and we should bear in mind that many charming and civilized people found him lovable.

University of Texas, Austin DAVID BRAYBROOKE

Configurations of Masculinity: A Feminist Perspective on Modern Political Theory. By Christine Di Stefano. Ithaca: Cornell University Press, 1991. 256p. $29.95 cloth, $10.95 paper.

"Gentlemen: this book's for you." It is not new to suggest that, despite their explicit address to a generically human condition, the canonical texts of western political theory may implicitly carry rather more partial dedicatory inscriptions. At least since Wollstonecraft—which is to say, well before the term *feminist* entered the English lexicon—writers of the female persuasion have repeatedly taken exception to the masculine undertones of that tradition. Focusing on the work of Hobbes, Marx, and Mill, Christine Di Stefano reiterates this criticism even as she significantly shifts and complicates the position from which it is voiced. At once a feminist intervention in political theory and a political intervention in feminist theory, *Configurations of Masculinity* is a provocative contribution to both.

Di Stefano draws upon object relations and literary theory to consider "the plausibility of a demonstrable connection between modern masculine gender and the political theories of Hobbes, Marx, and Mill" (p. 11). The connection suggested, however, as well as the approach used to elaborate its features in each case, distinguish this work from what should by now be familiar criticisms of the sexism of social and political theory. At issue in *Configurations* is not particular authors' neglect or exclusion of women nor even how the internal coherence of a particular theory is structurally guaranteed by the subordination of conventionally "female" activities. Instead, Di Stefano argues, masculinity functions in the form of culturally and historically specific "cognitive proclivities" structuring perception "such that the social world is interpreted, and hence engaged, contested, and reproduced, within the horizon of those perceptual commitments" (p. 55). More specifically, as formulated by object relations theory, masculinity is a deep cognitive orientation generated by the processes of gender differentiation in the modern nuclear family—in particular, by the distinctive tensions and ambivalences experienced by boys during the pre-Oedipal stage of separation and individuation from the mother. Characterized by rigid ego boundaries and an exaggerated emphasis upon separation and autonomy, it is at once supported and threatened by its differentiation from a female maternal "other"—which Di Stefano refers to as the "(m)other."

Mothers, to be sure, receive scant notice in modern political theory, but this is not Di Stefano's focus. In the book's most theoretically innovative move, she shifts beyond the question of explicit representations of women or femininity. Embracing the hermeneutic principle that reading *generates*, rather than finds, meaning, she adapts Teresa de Lauretis' use of the *space-off* (*Technologies of Gender*, 1987)—a concept in film theory designating that which is implied by the cinematic apparatus yet not visible within a film's explicit frame of reference—to situate her own project of rereading. Locating the "(m)other" of object relations accounts in this space beyond overt representation, Di Stefano theorizes maternal origins as that which is at once necessary to, and elided by, each of her selected theorists' productions of a generic humanity.

Di Stefano thus limns the political face of modern masculinity as a more general construction of the political across otherwise disparate theoretical frames. It is not, however, offered as a seamless or unitary epochal characteristic. Separate chapters devoted to Hobbes, Marx, and Mill identify "heroic," "productive," and "disciplinary" masculinity (respectively) as distinct variations on a common frame of reference. Each configuration, Di Stefano argues, offers up substantive and thematic tensions between images of autonomous self-creation, longings for the solace of social unity, and fears of engulfment or annihilation—tensions that resonate strongly with the object relations account of masculine identity as a precarious achievement—hence Leviathan's taming of "heroic masculinity," communism's emancipation of "productive masculinity," and Mill's dual quest for individuality and social progress through civilized self-discipline. Each theory, on the account offered here, sustains the illusion of generically human self-production by effacing its own gendered specificity. Simultaneously forgetting maternal origins and appropriating maternal power, each thus falls short of its own claim to a comprehensive account of the human condition—not by its treatment of women but, more profoundly, by its obliviousness to the masculinity of its own perceptual frame. In Di Stefano's trenchant phrase, "the 'forgotten self' of these political theories is the modern masculine self" (p. 16).

Configurations of Masculinity is a work of complex intelligence and subtle ambition, a work likely to challenge, rather than satisfy, its own multiple audiences of address. Three sorts of potential criticism bear mention. Those unconvinced that gender constitutes a distinctive politics may question the book's inattention to what are conventionally taken to be significant political stakes in the differences among Hobbes's sovereignty, Marx's class analysis, and Mill's liberalism. Similarly, feminists and fellow-travelers sympathetic to gender-based criticism of the canon may wonder whether such variations within modern masculinity may structure different prospects for reconfiguring gender relations. While Di Stefano's account does not foreclose either of these lines of questioning, neither does it address their respective concerns. Finally, readers invested in feminist debates over sexual difference may find themselves divided over the book's reliance upon the object relations account of gender differentiation. Arguably, this emphasis upon the pre-Oedipal roots of gender identity not only sustains the masculine–feminine opposition but does so at the expense of contemporary interrogations of sexuality. In effect, the identification of the (m)other as the off-screen presence of traditional theory may inscribe a different avoidance; for what occupies the "space-off," as it were, of Di Stefano's own account may be the sexual "other" elided by the constitutive heterosexuality of (re)producing mothers, an "other" now beginning to receive specific articulation in the emergent field of lesbian and gay theory.

Such criticisms, however, are beside Di Stefano's point—not in the sense of being irrelevant but, rather, in the sense of being parallel or proximate to the interpretations she advances. If she proffers the (m)other as a privileged site for feminist readings of canonical texts, she lays no claim to have discovered the truth of such texts' indebtedness to gender. Similarly, if important political differences among modern political theories appear to dissolve into diverse substrata of modern masculinity, the book is an invitation not to abandon such differentiations but to re-view them in light of their variegated imbrications with the problematic of gender. As emphatic in its insistence upon feminism's engagement with the canon as it is upon political theory's engagement with feminist perspectives, *Configurations of Masculinity* thus accomplishes the difficult task of addressing diverse constituencies with both clarity and grace. And this is no small achievement.

Johns Hopkins University K. M. McClure

Rights Talk: The Impoverishment of Political Discourse. By Mary Ann Glendon. New York: Free Press, 1991. 218p. $22.95.

The title and subtitle of this book nicely indicate its content. Mary Ann Glendon argues that Americans have become fluent in discussing and claiming rights but dangerously inarticulate in speaking of responsibilities, of limits on rights, and of the complex and fragile social setting in which rights have to be realized. Among the results is a tendency to formulate all social and political issues in terms of rights and to phrase rights claims in stark and uncompromising terms. Hence, political discourse tends to be reduced to confrontations about rights, with the nuances of the situations being debated left out of consideration and the interests of the competing parties formulated as absolutes. This makes it difficult either to understand political issues adequately or to negotiate their resolution.

Glendon is particularly concerned with how our preoccupation with rights leads us to neglect the social ecology on which our lives depend for their human substance. We envision the rights bearer as isolated and self-sufficient. Hence, in public deliberation, we neglect the network of families, neighborhoods, churches, and other groups below the level of government without which freedom is empty. Judges, like the rest of us, are habituated to thinking and speaking in terms of rights and are consequently inept when it comes to the threat that certain presumed rights, like that of a business corporation to shift one of its plants to another city, may pose to traditional ways of life that cannot readily be defended in terms of counterrights. Phenomena such as urbanization, bureaucracy, mobility, and mass culture tend to strip individuals to human atoms whose relations with one another are controlled wholly by the market and the state. In such circumstances, rights talk loses the social roots necessary to its moral authenticity. It comes to be less about human dignity than about "insistent, unending desires" (p. 171).

To appreciate Glendon's argument, it is essential to understand that she is not arguing anything as banal as the necessity of balancing rights with responsibilities. She is concerned with what happens to our understanding of rights when we forget about responsibilities. Ironically, if we can think only of rights, our ability to understand and defend those rights is threatened. We become preoccupied with our own rights and lose our capacity for respecting the rights of others and for understanding the social circumstances in which rights are realized. The purpose of the book, accordingly, is not that of curtailing rights talk but of refining and deepening it.

In a sense, *Rights Talk* is about law; and it contains informative discussions of court cases in this and other countries. But when the author is discussing law, she obviously has in mind also morals, politics, and language, and one of the major virtues of her book is the way it illuminates the role of law in the overall life of society. It shows, for example, how legal and moral norms interact and also how litigation shapes our language and thus affects the way we talk about, and act upon, our social environment. A particularly interesting example is her discussion of "silences" in the law. Given the heterogeneity of contemporary society, it is often hard to identify values that we all have in common. This gives law a particular moral weight; people tend to assume that an obligation not prescribed in statutes and upheld in the courts is not an obligation at all. Thus, the lack of a legal obligation, such as that of coming to the assistance of someone in peril (even the police in America being under no such obligation), encourages people to infer the lack of a moral obligation.

This is not an easy book to criticize. The author's argument is continuously interesting and is upheld with a wealth of information from this and other countries. The main themes advanced seem both true and important. And almost everything that is said is said felicitously and exactly. In the most comprehensive sense of the term, it is an intelligent book.

One might, however, take issue with Glendon's suggestion that there exists in the United States "a significant constituency . . . for candor, moderation, and complexity" and that what is in doubt is only "whether Americans in positions of leadership have the will, ability, courage, and imagination necessary to respond to and mobilize such a constituency" (p. 183). One can hardly deny that American leaders on the whole are both simplistic and timid in their public utterances. Nevertheless, they have gained office by being elected and one of the main reasons they do not speak more boldly is that they have learned better. For example, any candidate with even the modest respect for complexity necessary for saying that perhaps taxes need to be raised rather than lowered or that there may be legal ways of recognizing the evils of abortion without interfering intolerably in the private lives of women will, unless the audiences for such suggestions have been very carefully chosen, quickly be taught the perils of candor and moderation.

But this is perhaps only a quibble. What mainly needs saying is that this is a book that specialists in political theory and public law—and indeed all branches of political science—are certain to find provocative and instructive.

University of Massachusetts, Boston GLENN TINDER

Political Theory Today. Edited by David Held. Stanford: Stanford University Press, 1991. 360p. $39.50 cloth, $12.95 paper.

This well-edited volume offers 13 chapters of high quality essays ranging over numerous issues in contemporary political theory. Its most distinctive strength is a set of five essays on international politics: David Held's on democracy, the nation–state, and the global system; Charles Beitz's on sovereignty and morality in international affairs; Antonio Cassese's on violence, war, and the rule of law; Onora O'Neill's on transnational justice; and Samir Amin's on the state and development. To those acquainted with these authors' previous work, there will be little that is dramatically new; but each manages to restate key theses in a lucid and organized manner. Four essays represent sophisticated versions of conceptual analysis conducted with sensitivity to history and empirical political science: John Dunn's on political obligation; Steven Lukes's on equality and liberty; Andrew Reeve's on property; and Agnes Heller's on the concept of the political. Writing from a rational choice perspective, Iain McLean contributes an essay on forms of representation and systems of voting, while Jon Elster offers cogent if not exactly unfamiliar reflections on the limits of rational politics. Susan Moller Okin analyzes the public/private distinction from the perspective of gender, while Clauss Offe and Ulrich Preuss explore how democracy can be justified, limited, and extended.

Two other strengths of this volume should be noted. First, most of the chapters are written with admirable breadth and grasp of the relevant literature. Reeve's and O'Neill's contributions are exemplary in this regard, amounting to concise review essays that scholars will wish to consult early on. Second, taken as a whole, the chapters represent a welcome engagement of theory with the real world of politics, along the lines long recommended by Brian Barry, John Dunn and others. That the terms of this engagement are so manifestly uncertain and contested is only an argument for proceeding further.

The volume begins with a useful and balanced editor's introduction exploring the contemporary revival of political theory under the rubrics of its "object" and "nature." With regard to objects, Held argues persuasively that the traditional focus on the public institutions of the nation–state must be broadened both *internally*, to cover a wider range of economic, social, and familial activities, and *externally*, to address not only changing relations among nation–states but also new issues and institutions that cut across their boundaries and powers. With regard to the nature of contemporary political theory, Held delineates seven principal approaches: history of political thought, conceptual analysis, systematic theories of political value, concrete problem- or institution-oriented explorations, discussions concerning foundationalism and postmodernism, systematic model building, and theory directed toward empirical political inquiry.

It is of some interest to look at Held's volume through the prism of his own typology. The foundationalist/antifoundationalist, communitarian particularist/liberal universalist, and modern/postmodern debates go unrepresented. Also conspicuous by its near absence is systematic value theory. There are, for example, no sustained discussions of domestic distributive justice or of deontological liberal theory: John Rawls is far less central than in other easily imaginable volumes on contemporary theory and, to judge from this volume, utilitarianism is all but dead. There is also surprisingly little on the theory of the welfare state, about which (as David Miller argues) there has been remarkable ferment in Britain and rising concern in the United States as well. Nor is there much about the kinds of social issues—multiculturalism, difference, affirmative action, dependency, and others—that have so dominated recent public debate and (increasingly) political theorizing in the United States. Nor (with the partial though honorable exception of the Offe and Preuss chapter) is there sustained attention to the individual moral prerequisites of citizenship or, conversely, the moral consequences for individuals of participating in various kinds of institutions.

This enumeration, which could be lengthened, is not intended as a criticism of the volume under review, which represents a serious effort, stretching over 360 pages of dense type, to achieve breadth of coverage. It is to say, however, that a volume fully representing "political theory today" would have to be much longer and far more diverse. In the face of the proliferation of political theory over the past 20 years, perhaps no single volume can any longer aspire to such representativeness.

There is another reason why this may well be the case. In many respects, political theory is politics carried on by other means. One would expect, therefore, that political theory would tend to mirror the full range of contemporary public debate. *Political Theory Today* presents the portion of that range running (roughly speaking) from liberal egalitarianism to quasi Marxism. I had thought that over the past decade in the United States and Britain—indeed, in Europe from the Atlantic to the Urals—other political tendencies were finding public voice and systematic expression. They, too, deserve to be considered part of "political theory today."

University of Maryland, College Park WILLIAM A. GALSTON

Politics/Sense/Experience: A Pragmatic Inquiry into the Promise of Democracy. By Timothy V. Kaufman-Osborn. Ithaca: Cornell University Press, 1991. 327p. $21.91.

John Dewey and American Democracy. By Robert B. Westbrook. Ithaca: Cornell University Press, 1991. 570p. $29.95.

Something of a "Dewey revival" is under way. These two books come on the heels of Richard Rorty's decade-long campaign to get American scholars to take Dewey as seriously as they have long taken Wittgenstein, Heidegger, and Nietzsche, while they coincide with Steven Rockefeller's study of Dewey's religious thought (*John Dewey: Religious Faith and Democratic Humanism,* 1991) and Robert Bellah and his colleagues' call for a revival of Deweyan "public philosophy" in *The Good Society* (1991).

Dewey occupies a slightly odd place in American intellectual life. When he died in 1952, he was an intellectual dinosaur. He had been a philosopher in the grand style, ready to write about art, religion, morals, politics, and the nature of science, and sometimes about all of them at once. He died when the analytical movement's reaction against all this was at its fiercest; and although he was honored as America's greatest philosopher, he exerted little influence on the subject for the next 30 years. The revival in his reputation even now owes less to the discovery of his merits by orthodox academic philosophy (though there is some of that) than to the sense that the analytical enterprise has lost its way.

John Dewey and American Democracy offers the reader very much what the title suggests. Westbrook takes us through Dewey's career step by step, tackling his views on democracy and education, industrial democracy, the religious sensibility of a democratic society, the role of art in a democracy, and more. The author writes with the enthusiasm of a convert. A note confesses that once he shared Christopher Lasch's view that the main function of pragmatism was to teach young Americans how to be well-adapted citizens of the corporate state; but now he sees Dewey as an inveterate radical, a critic of democratic practice in the light of democratic ideals, a social democrat, and an enthusiast for workers' control. His enthusiasm for his subject greatly enlivens this substantial book and ought to persuade any reader that there is much in Dewey still worth reading.

Politics/Sense/Experience is not so much "about" Dewey's views on democracy as a Deweyan meditation on the domination of our lives by a narrowly instrumental rationalism and the contrast between this and the politics of a truly democratic society. Because what Kaufman-Osborn values most highly in Dewey is the latter's ability to subvert or displace the conventional meanings of terms, it is not quite exact to say that *Politics/Sense/Experience* criticizes contemporary politics in the light of Deweyan ideals. Rather, the deficiencies of contemporary politics are seen as an outgrowth of a narrow conception of how rationally to manage our affairs. The defects of this conception are illuminated by an appeal to Dewey's own view of the way *experience,* with its connotations of doing and suffering, had been denigrated in favor of *reason,* understood in essentially spectatorial, detached, unengaged terms. "Political things" lose their meaning when detached from experience, and we are suffering from just that meaninglessness.

Westbrook's book is an admirable piece of work. Now that we have Dewey's *Collected Works* in 37 volumes, it is easier to appreciate the sheer hard digging that Westbrook engaged in before the Dewey Center at Southern Illinois University had finished its work. He seems to have read everything by and about Dewey and most things by and about Dewey's contemporaries. Particularly valuable is his way of moving in and out of Dewey's wider philosophical concerns, as well as his politics (though that is a slightly misleading way of putting it, since Dewey insisted that the kind of cultural criticism that philosophy had become in the twentieth century was a seamless web).

It is only by following Dewey through the whole enterprise that one can understand why he enjoyed the esteem he did. His claim that there need be no conflict between faith and science, high culture and democratic politics, art and democracy was just what his readers wished to be told. Westbrook usefully conducts us through Dewey's account of religion, with its startling conclusion that in the modern world, the task of the churches is to vanish, leaving behind a common faith in democracy and a conviction that its pursuit is overwhelmingly valuable. Again, it is not quite clear either how Dewey's relations with the eccentric Albert Barnes resulted in *Art and Experience* or how Dewey's belief that in a democracy all experience must have an aesthetic quality relates to Barnes's hatred of art experts and his enthusiasm for teaching art history to ordinary folk; but it is clear that Dewey thought a democracy could be more, rather than less, appreciative of high art.

Admirable as *John Dewey and American Democracy* is, its governing theme is not very persuasive. Westbrook writes of Dewey as a participatory democrat and an enemy of corporate liberalism, quite as if Dewey was the patron saint of Students for a Democratic Society. This must be multiply misguided, especially in its suggestion that Dewey was hostile to liberalism. Certainly, he thought capitalism doomed during the 1930s; certainly, he thought the rise of the large capitalist corporation had made the economy ungovernable. Certainly, he wanted some form of industrial democracy, vague though he was about just what it was. But he never renounced his own liberalism. Nor was his conception of the participatory society on all fours with the blend of Marx and Rousseau that we were fond of 20 years ago; quite what it was is hard to tell, but it certainly was not to be found in the Port Huron Manifesto.

Anyone with a certain amount of philosophical training will also wonder why Westbrook is so uncritical of his subject. Dewey's conviction that democracy and education are mutually reinforcing, for instance, is on the face of it a sleight of hand, achieved by emphasizing the "organic" quality of understanding and the "organic" quality of democratic life. Westbrook never stops to wonder whether Dewey's reconciliations rest on anything firmer than definitional fiat. Again, Westbrook uncritically accepts Dewey's attempts to preserve the "religious" attitude while turning his back on "religion" but is much too quick to dismiss Reinhold Neibuhr's complaint that in throwing out institutional Christianity and the doctrine of original sin, Dewey ignored the radical imperfection of human nature and our need for constant moral discipline.

Dewey used to get particularly irritable whenever Bertrand Russell criticised his residual Hegelianism, and Westbrook is still irritable on Dewey's behalf. But Russell was quite right to ask why Dewey was so confident that "experience" revealed a world permeated by meaning and by the values to which Dewey subscribed. Dewey had been a Congregationalist, and then an Idealist, before adopting his own kind of naturalism. It was proper to ask what in naturalism substituted for God and the absolute, and *nothing* was hardly an answer. Still, Westbrook writes with such lucidity that readers will have no trouble seeing when Dewey is skating on thin ice.

Kaufman-Osborn is more elusive than Westbrook. Readers who share his horror of Weber's "iron cage" and believe that we are subject to the tyranny of instrumental rationality will find him persuasive; skeptics who think we are more alarmingly subject to the whims and follies of our rulers and in need of a lot more instrumental rationality will not. Those who share his view (held by the young Marx and many other distinguished thinkers) that "politics" has been stolen from the people will find him persuasive; skeptics who have always wanted to know what we would do with politics when we got it back and have found no answer in Marx, Arendt, Sheldon Wolin, or Jürgen Habermas may complain that they find no answer here. I find my heart with the sympathizers and my head with the skeptics. It was at least somewhat unwise of Kaufman-Osborn to couch his meditation on these difficult issues in a Deweyan voice. It adds needless complexity to what is already vertiginously difficult, and Kaufman-Osborn's own view of our situation is quite interesting enough for him to risk giving it to the reader straight.

Princeton University ALAN RYAN

Legal Theory, Political Theory, and Deconstruction: Against Rhadamanthus. By Matthew H. Kramer. Bloomington: Indiana University Press, 1991. 352p. $35.00.

Kramer's book proceeds on two levels. The larger and more successful part of the book is devoted to textual analysis of some of the key writings of G. A. Cohen, H. L. A. Hart, David Hume, and members of the Critical Legal Studies movement. A smaller and less fully realized portion of the book is concerned with negotiating the transition between theory and practice—how its textual strategies might enable us to achieve a bolder, and less frustrating, radical politics.

In his introduction, Kramer defines as the book's principal aim "to make clear the relevance and power of deconstruction for analytic philosophers" (p. 37). In this he succeeds superbly. One of the great virtues of the book is its systematization of the multifarious writings of Jacques Derrida and their distillation into a critical canon that is imaginatively and illuminatingly extended to some standard texts in legal and political theory. Central to this canon is a relentless working out of the implications of dilemmas surrounding self-referentialism: "If Epimenides avows that all Cretans never make truthful statements, and if he is himself a Cretan (and if all Cretans thitherto have indeed been chronic liars), then his pronouncement will be true only when false and false only when true" (p. 20). This perspective immediately exposes the vulnerability of extreme skepticism and relativism in philosophy. If extreme skepticism is true, then it is false. The same conundrum is applicable to extreme relativism as well.

The issue of self-referentialism also helps to structure a number of other antinomies relating both to our sense of the world and our use of language (p. 34). One of these has to do with how events can be individuated only in relation to a particular structure; and yet the structure as a whole can only be individuated in relation to the events that compose it. Analogously, the framing of events or texts through interpretive statements both constitutes an attempt at closure and attests to the impossibility of ever achieving closure. Every interpretive statement needs, in turn, to be interpreted and thus everlastingly forestalls what it presumes. Also, "because signifiers are always subject to recontextualization," the prospect of an assymetry—even a clash—between the "codes of intentionality" and the "codes of construability" always remains open and undermines the certitude of assigning consistent truth values even to tautologies (p. 29). Derrida extends the instabilities disclosed by paradoxes of self-referentialism into the citadel of language itself. With his famous notion of *différance,* Derrida calls attention to how the play of signifiers is grouped to yield significant meaning only in relation to receding contexts of what is other than itself, so that irresolvable problems of regress and circularity that were diagnosed in relation to the event/structure and text/interpretation dichotomies resurface with regard to the dualism of signifier/signified. There is an irremediable opacity in our very use of language: "All positions—be they metaphysical or political—will perforce dismantle themselves as the condition of their being elaborated" (p. 148). Kramer has a veritable field day transposing Derridean deconstructive moves to texts in legal and political theory. Thus, he shows how the distinction between *materiality* and *sociality,* pivotal in Cohen's interpretation of Marx, is undermined by the fact that *materiality* is "always already social" (p. 87). What is classified as the material domain is mediated through a set of linguistic and cultural categories that is socially ordained. In his analysis of Hart, Kramer shows how an infinite regress derails Hart's search for normativity. Hart's vaunted "rule of recognition," supposed to be internal to every legal system, can confer legitimacy on the rules that it pinpoints but cannot endow itself with binding force without erupting into an infinite regress (p. 143). Hume's philosophy unhinges itself in ways that resonate directly with Derrida's concept of *différance,* because the sensory particulars that are the building blocks of Hume's empiricism get their individuation endlessly deferred by the unceasing play of difference between what is and what is not themselves (p. 203).

Kramer fares less successfully with the second, more subordinate, part of his project, namely to evolve a more coherent radical politics from what is currently available, on the basis of Derridean deconstructive principles. He is very sensitive and sophisticated about the difficulties of effecting transitions between metaphysics and politics (pp. 246–47). Nevertheless, in his long, concluding chapter, he engages in an uphill battle to achieve a fruitful political "clearing" in deconstructionist theorizing. Here, I feel, Kramer is less successful than in the rest of the book and is joined by the company of the vast majority of postmodernist theorists who are unable to coherently and fully articulate the connections between their metaphysical positions and their politics. Kramer is writing against Rhadamanthus—against objectivity. The deconstructionist approach he pursues, with its continual disclosure of *aporias* and paradoxes in both sides of traditional dichotomies such as skepticism/knowledge and inner/outer, ends up resurrecting a version of objectivity—a structure of dynamic "thereness" that remains unimpugned by the persisting recuperative turnings of deconstruction. The structure of the real is diagnosed as a field of dialectically countervailing forces, but it is still the "structure of the real." Deconstructionism discounts all discountings (including those of a Freudian and pragmatist sort), so that the remainder of its argument is what was there before—the brutely given, the sheerly and everlastingly unfolding other, the real. For all its sophistication, therefore, deconstructionism appears to reconcile us to what is. In the end, it is a philosophy of acquiescence—of dealienated resignation before the world (no matter how subjectively/objectively defined). Perhaps Kramer needs to take a cue from a critical comment he himself makes at the beginning of the book in talking about liberalism: "Those who hope for assurance that a tyranny cannot ever arise will have to plummet into an infinite regress, wherein each ground for encouragement has reproduced all the insecurities that evoked it. Each layer of supervision will provide assurance about everything but itself. Literal theories of politics hence turn out to be structured by incompleteness, since they enact the impossibility of achieving full and rigorous closure" (p. 22). Perhaps, then, the refusal of theory—unmoored from requirements of justification—becomes the most stalwart and clear-headed "theoretical" stance to take with regard to political matters.

Temple University ARYEH BOTWINICK

Equality and Partiality. By Thomas Nagel. New York: Oxford University Press, 1991. 186p. $17.95.

Very early in this difficult and illuminating book, Thomas Nagel remarks, "We really do not know how to live together." By this he does not mean only, or even primarily, that we live in a world of fractious disagreement. The deeper point is that we have not yet discovered social and political forms capable of ensuring the satisfaction of the claims we are justified in pressing upon each other without simultaneously imposing burdens on ourselves that we could reasonably resist. Nagel takes the reconciliation of this conflict between the perspectives of the collectivity and of the individual (the "equality" and "partiality" of the title) to be the main problem confronting political theory. It is also a main problem for political practice; and the very low probability of its being adequately resolved in practice anytime soon makes the theoretical enterprise a gloomy affair indeed.

The contrast of perspectives is a reflection in ethical thought of an inherent duality in human nature—the capacity to regard the world, in turns, from a particular position within it and from a position detached from it. Looking at things from the particular point of view of a determinate self, the success of one's own life will appear to be of very great importance. Looking at things from the detached point of view of one self among many, it will seem that the success of each life matters equally. Each point of view can give rise to reasons for action, and these reasons may conflict: impartiality may require more than people can give without risking the success of their own lives. When such a conflict exists, there may be no comfortable resolution, because it can be reasonable to be motivated by the requirements arising from both perspectives.

This complexity in practical reason will be familiar to readers of Nagel's earlier works, in which it is treated as a central problem of ethical theory. Here, it emerges as a problem for political theory, as well. In fact, it is part of several problems, of which I shall mention three that preoccupy him in most of this book.

First, and perhaps most fundamentally, it is a problem for the theory of legitimacy. The task of political theory is to discover the nature of legitimate institutions. Nagel's conception of legitimacy reflects the Kantian notion that morally acceptable institutions should be unanimously acceptable to those who live within them. To use the formulation Nagel borrows from T. M. Scanlon, institutions should conform to principles which no reasonable person could reject. The chief interest of Nagel's discussion of this difficult subject is his reinterpretation of the Kantian test in light of the two standpoints. His position is, perhaps, more controversial than he allows; but even those who disagree with this interpretation of legitimacy will find it a fruitful development in contractualist thought.

Second, the contrast of standpoints sets a problem for our conception of the role of political institutions in individual life. In recent years, political theorists influenced by John Rawls's *A Theory of Justice* have concentrated on matters of distributive justice in the broadest sense, that is, on how social institutions distribute the benefits and burdens of social cooperation. Although Nagel pays considerable attention to such questions here, his main contribution is to shift the focus to the interplay of legitimate institutions and the moral lives of individuals. Even in a world in which the requirements of distributive justice had been fully realized, the two standpoints could come into conflict. Institutions should help reconcile this conflict by satisfying the requirements of impartiality through collective action, so that individuals are left free to live their own lives in relative freedom from the direct demands of others. By doing their share in a moral division of labor, legitimate institutions become essential conditions of each individual's moral life.

The third problem involves moral motivation. Nagel believes that most of the inequalities of wealth and status characteristic of modern societies (to say nothing of those characteristic of the world at large) are unacceptable when these societies are regarded from an impartial point of view. Radical changes in institutions would be necessary to reduce social inequalities to acceptable proportions. As if the motivational obstacles confronting the transition were not enough, Nagel argues that maintaining egalitarian institutions once they were established would require large and perhaps inconceivable changes in human motivation, as much in the political sphere as in the economic. Largely for this reason, he claims that an ideally egalitarian society is difficult to imagine and, in any case, practically out of reach. The best we can hope for is something like progressive social democracy. Nagel regards this alternative as distinctly second best; but even so, achieving and sustaining it will be a daunting challenge to the motivational capacities of people in most industrial societies.

There is much in this short and unsettling book to stimulate serious thought about a wide range of issues in contemporary political theory. Nagel's style is deceptively simple, masking an unusual depth and complexity of view. This is not a book for philosophical amateurs. For those who have read widely in the recent literature of political philosophy, it is a rare treat.

Bowdoin College CHARLES R. BEITZ

Democracy and Moral Development. By David L. Norton. Berkeley: University of California Press, 1991. 198p. $29.95.

Since its inception, liberal political theory has been criticized for positing moral ideals that have not been fully realized in practice. In recent years this line of argument, most closely associated with demands for social reform, has been overshadowed by criticism of the ideals themselves. So-called communitarians have decried liberal theory's removal of the individual from a meaningful social context and consequent postulation of an overly abstract conception of the right, regarded as prior to the good. Other critics have appealed to the virtues of the ancient Greeks and their belief in an objective human good, to the realization of which political life should be devoted.

David L. Norton has feet planted firmly in both of these camps. He describes liberal theory as an "ethics of rules" and criticizes it for moral minimalism in two senses. It regards only certain subjects as moral, and prescribes rules in regard to these that hold generally and so cannot require special or developed moral characteristics of their upholders (p. 21). Norton's eudaimonistic moral view, which he calls an "ethics of character," is based on the Aristotelian view that the good life consists of the full development of one's moral and intellectual faculties (intro.). Like J. S. Mill, Norton believes that individual development depends on making one's own choices, discovering one's true nature for oneself. In opposition to liberal individualism, which opposes the individual's self-interest to the interests of other people, Norton argues for an expanded conception of self-interest, which encompasses the interests of others.

Throughout his book, Norton draws from an impressive array of sources, including Plato's Socrates, Mill, Dewey, Emerson, Tocqueville, Maslow, and Oakeshott. His discussion is most valuable in providing a coherent framework for a range of important issues, both theoretical and practical, and in proposing concrete steps for implementing his ideas. Like Mill and John Dewey, Norton believes that active self-government should play an important role in self-development and criticizes "equilibrium democracy" upheld by many contemporary theorists (chap. 2). In the spirit of Dewey, he recommends restructuring education (e.g., by alternating academic subjects and practical experience) and supports youth public service (chap. 3). As one can imagine, Norton advocates meaningful—as opposed to alienated—work, through which, once again, one's potential could unfold (chap. 4). He takes a broadly communitarian view of the individual's relationship to society (chap. 6) and argues that government, along the lines of Plato's *Republic*, should concentrate on helping individuals to realize their potential (chap. 7). Norton's most sustained philosophical discussion concerns a view of rights based on responsibilities, rather than welfare (chap. 5). Rights should be founded upon what one requires in order to develop properly. Viewed from this perpective, rights, too, blunt the adversarial thrust of traditional liberal theory.

The main problem with all this is that Norton says little about the ideals we are to realize. He does declare that all individuals possess all the virtues *in potentia* (p. 83). Under the proper conditions, with their potential made actual, all would lead meaningful lives, in a society devoid of conflict. Virtually nothing is said about competitive market economies or the economic consequences of proposed educational and vocational reforms. Norton realizes the need to abandon the "closed teleology" of Plato and Aristotle for "a liberal open-ended teleology whose ends require to be discovered through exploration" (pp. 6, 163). But the only definite value he upholds is moral autonomy. In regard to this, one must ask, Autonomy to what end? Without a clear account of the good, Norton's depiction of worlds of meaningful education, meaningful work, and meaningful community are cloying in tone and largely devoid of substance. Indicative of the remorseless hopefulness that suffuses his book is Norton's interpretation of Plato's *Republic* as upholding a political system in which the rulers' power depends upon the consent of the governed (p. 160).

Norton is correct that contemporary liberal theory pays more attention to the public than to the private, to rules of justice than to desirable traits of character, to the right than to the good. But the reason for this is the need to provide a stable framework within which people of diverse moral, religious, and political views can live together in harmony. Norton's open-ended eudaimonism would do little to obviate the need for this task. In emphasizing responsibilities over rights, the place of others' happiness in our own self-interest, and the importance of developing our latent powers, Norton has something important to say. But without a clear account of the good (or goods) that all citizens should pursue, his views provide a supplement, rather than an alternative, to existing liberal theory.

University of Virginia GEORGE KLOSKO

The State: Its Nature, Development, and Prospects. By Gianfranco Poggi. Stanford: Stanford University Press, 1991. 214p. $29.50 cloth, $9.95 paper.

Gianfranco Poggi is an experienced, learned, and sophisticated political sociologist who has authored several valuable works on political theory. *The Development of the Modern State* (1978) was an impressive short history of the rise of the modern European state. Not only was it interesting for scholars because it summarized so much European, particularly German scholarship, but it was also extremely valuable as a classroom text. Undergraduates found it difficult; but it introduced them to such concepts as feudalism, the *Ständestaat*, absolutism, and liberalism in a neatly comprehensible way.

This new work is meant to add to the earlier, successful volume. It is a more difficult, and also a more rambling book because it grapples with Poggi's rather mixed assessment of the direction of modern politics in Europe and North America. Perhaps it tries to cover too much by providing introductory material, a review of several European lines of theoretical thinking about the state, and a good bit of personal opinion.

Poggi begins by reviewing the history of the rise of the European state. He places insufficient stress on the survival of the *Ständestaat*—particularly in England, but in the Netherlands, as well, as the precursor of the liberal state. Continental absolutism, whether of the French, Prussian, Habsburg, or Russian type all proved to be dead ends, though it took two centuries (1789–1989), three bloody world wars (the Napoleonic Wars and World Wars I and II), and the intervention of England's heir (the United States) to save the liberal state. It is true, as he points out, that the modern liberal state is a vastly more powerful entity, and controls a much larger portion of our lives, than it did in the eighteenth century. But it is an error to see the modern liberal state as derived from European absolutism. Not only Montesquieu but much of the aristocratic opposition to absolutism (and it was aristocratic, not bourgeois, opposition, that began the French Revolution) looked to England precisely because it had preserved an important balancing role for its elite estates. How the United States managed to preserve some of this through its complex constitutional arrangements, at least until quite recently, is the part of the story that most European theorists tend to miss, though Americans (e.g., Samuel Huntington) know it well. The more modern, efficient, centralized bureaucratic systems of continental Europe, combined with the rise of mass participation in politics and of an egalitarian ethos produced totalitarianism, not the modern liberal state. Only the Anglo-American victory in 1945 and the subsequent defensive wall erected against communism in Europe kept continental Europe liberal. And all totalitarian systems have proved to be too inflexible to adapt well to the demands of rapidly changing technologies.

Poggi then goes on to describe the development of the modern state and to stress the disjunction between increasing democracy and the growth of specialized bureaucracies. Again, his summary and review of important works on this topic are elegant and useful. But in the end, he cannot explain why, in the past decade, there has been a revolt throughout Europe and North America against this old trend of bureaucratization and reliance on specialized civil servants to run the state. This has gone much further in the United States than in Europe, probably because the United States never went through an absolutist royal stage. Here, for reasons that are not very clear, the civil service has lost its legitimacy. It remains large and powerful but also underfunded and despised by most of the population. This has produced a kind of vicious circle in which the quality of high civil servants (those whom some Europeans like to call "mandarins") has declined, the efficiency of key portions of the bureaucracy has degenerated, and contempt for it has increased.

It is sobering to read Poggi's chapter on Gorbachev and the Soviet Union, in which he explicates the failed Soviet system as an alternative to the liberal state. Since he wrote this section, the centralized Soviet bureaucracy has virtually disintegrated. Ironically, the extreme expression of European absolutism—the Stalinist state—has failed at the same time that its arch enemy, the United States, the great bastion of liberalism, faces the incipient collapse of large parts of the central bureaucracy. That shows the advantage of preserving old-fashioned local and private centers of economic status and political power. In a liberal society, even one as degraded as the United States, the failure of the central bureaucracy leaves much of the rest of the society functioning tolerably well. In an absolutist state, the collapse of the bureaucracy leaves only chaos. In his next book, Poggi may take on these large questions, which he introduces, but does not fully elaborate in this volume. There are few comparative political sociologists more able to accomplish this task than he.

University of Washington, Seattle DANIEL CHIROT

Value-Free Science? Purity and Power in Modern Knowledge. By Robert N. Proctor. Cambridge: Harvard University Press, 1991. 331p. $34.95.

The question whether science can or should be "value-free" is one of those perennial topics on which it is difficult to say anything that is both new and sensible. A primary cause of the failure to be sensible is the tendency in such discussions to reify the critical terms, especially *science* and *value*. We blithely assume that there are two distinct entities, science and value, and ask how they are related to each other. But the real issue is always how science and value and other related concepts (e.g., fact, objectivity) are understood or constructed. What is at stake in these discussions, as Proctor clearly shows, varies with the intellectual, social, and political context in which they occur. In the ancient world, in early modern thought, in the great *Werturteilsstreit* of German social theory at the turn of the century, and in post-1945 American social science, different meanings were attached to the key terms, and different issues were at stake. Thus, members of the Royal Society in the seventeenth century may have been concerned to defend their investigations against religious censorship, while postwar American social scientists reacted against Nazi efforts to claim scientific warrant for their monstrous projects. Because standard accounts of the relationship between values and science are conducted on too philosophical and too abstract a plane, Proctor argues, they fail to realize the complexity and the variability of these issues, and an historical approach is required. By explicating the arguments of the proponents of value-freedom, Proctor clearly demonstrates how this set of issues has changed, reflecting the intellectual, moral, and institutional pressures and concerns of different times.

Many of the individual chapters addressing particular thinkers and their contexts are illuminating and reflect a sure grasp of the relevant scholarly literature. But, perhaps because it covers such a wide range of cases, the work is sometimes an unsure guide to the territory. In some cases, for example, the argument becomes tendentious, to the point of deliberately misreading his sources. Citing Tönnies, he argues that neutrality "was proposed in opposition to socialism, the expansion of women's rights, and the mixing of races"; yet he quotes Tönnies as saying, "As sociologists, we are neither for nor against *socialism*, neither for nor against the *expansion of women's rights*, neither for nor against the *mixing of races*" and adds that sociology's task is not to further or obstruct particular ideas or movements (p. 92). Similarly, on the basis of his examination of the views of prominent defenders of the ideal of value freedom (Sombart, Simmel, and Weber), Proctor argues that the commitment to value-freedom functioned to exclude women from science. But it is far from clear that their beliefs about value freedom (as opposed to beliefs about women's alleged capacities) had anything whatever to do with their attitudes toward women is engaging in scholarship. Indeed, they reached significantly different conclusions in spite of the similarities in their commitments to value freedom.

In other cases, important errors creep into the argument. In the chapter devoted to Weber, for example, he fails to recognize that Weber's view of value-freedom is qualified by his recognition that "ideal-types" are constructed with an eye to their "cultural significance": different investigators, working from different points of view, will select different traits in constructing "an ideal-typical view of a particular culture" (*The Methodology of the Social Sciences*, 1949, p. 91). Proctor also appears to confuse Weber's ideal-types with the "idealizations" of the physical sciences (p. 146). In a similar vein, we are also told that "the positivists" believed that "methodology must be closely linked with subject matter" (p. 146), though we later learn (this time closer to the mark) that positivism is committed to "an assumption of the unity of science" (p. 159).

More seriously, however, the work loses its way in forgetting its primary lesson, that the call for value-freedom in science is not one thing but means different things and that it is advocated for different reasons in different contexts. Although granting that the ideal of neutrality was once "progressive," it no longer is so, Proctor asserts, because "science has become a power in itself" (p. 270). Warming to his theme, he argues that "all science is politics, or ethics," because "science *should* respond to practical problems of human need and suffering" (p. 270). But if this work teaches anything, it is that *science* is not a unified agent to which power and responsibility can be ascribed. Proctor dismisses the ideal of neutrality on the grounds that "the priorities of science are shaped by larger social priorities" (p. 267). But few advocates of value-freedom would disagree with that observation. The problem is to organize the practice of, and to set priorities for, research without compromising the objectivity—or, to use an even more old-fashioned term, the truth—of the results; it is to guard against the kinds of abuses epitomized by Lysenkoism and Aryan physics. Theory, as Proctor argues, can "shape the world" (p. 230), in part because research can lead us to alter what we believe and how we act in unexpected ways. And precisely because of that, the course and outcome of research can never be fully determined or controlled. Whatever else they advocated, the proponents of value-free science understood that point and, with it, the limits of Proctor's claim that "science is politics."

Wesleyan University J. Donald Moon

Post-Modernism and the Social Sciences: Insights, Inroads, and Intrusions. By Pauline Marie Rosenau. Princeton: Princeton University Press, 1992. 229p. $35.00 cloth, $12.95 paper.

Political Theory and Postmodernism. By Stephen K. White. New York: Cambridge University Press, 1991. 180p. $39.50 cloth, $12.95 paper.

At a decade or so past midcentury, with the dissemination of the writings of John Austin and Ludwig Wittgenstein on language, the once secure epistemological status of statements was called into question. Apart from the impact on philosophical discourse, the resulting blurring of the line between descriptions and other linguistic acts and the evocation of a vast domain of normative conventions intervening between statements and their referents, acts, and effects put tremendous pressure on many of the conceptual props of political theory and the social sciences in general.

There were many reactions, but here I refer to two very prominent and polar ones. One kind of response took the challenges seriously and attempted to rethink such domains as the history of political theory, the status of normative versus empirical judgments, the epistemology of the social sciences, and so on. The second was a defensive posture that regarded the texts with shallowness and hostility and reproached Austin and Wittgenstein's conceits as dangerous attacks on morality and "the real." These tended to be obtuse to the problem in that their reproaches made use of the very distinctions that Austin and Wittgenstein's analyses had called into question.

Now, as we approach the end of the century, a variety of philosophical, historical, literary, political, and critical/social analyses called postmodernist have offered a new set of challenges. It is important to note that the challenge has gone beyond the Austinian–Wittgenstinian destabilizing of the warrants for treating statements and has destabilized the grounds for evaluating "discourses"—the collections of narrative, rhetorical, grammatical orientations (sets of statements in various enunciative modes) through which fields of practice are organized and the subjects and objects of discourse are situated. The implications for political theory, political science, and the social sciences, among other domains of inquiry, have been far-reaching.

Stephen K. White's *Political Thought and Postmodernism* represents the first kind of reaction. It is an intelligent book; and although it is an assembly of chapters whose prototypes appeared separately, it is a coherent piece of analysis. It is held together by two of White's compelling interests: (1) his Habermasian inclination to save a model of intersubjectivity within which community can be negotiated rationally (he chides postmodernists for "cavalierly dismissing Habermas") and (2) his Heideggerian hope for a frame that would unite the critical interpretive interest in disclosure with a political–communitarian interest in norms for responsible action.

Convinced that Heidegger has left a legacy for postmodernism, White argues that Heidegger's concept of *Gelassenheit* overcomes a sharp dichotomy between passivity and activity and offers hope for moving from the critical process of disclosure to the normative legitimation for action. These strong positions give White's book both its strengths and weaknesses. On the positive side, White's determination to find in Heidegger a chance to bridge criticism and action and thereby vindicate a

Habermasian version of enlightenment reason is consistently argued against various postmodernist challenges that he poses to his hopes and views. In chapters on Heidegger and various postmodernist thinkers and in more thematic treatments of "difference feminism" and the problem of justice, White's treatments of postmodernist positions versus his own are nuanced, fair-minded, and scholarly.

The book's weaknesses, however, stem from its strengths. Because it is primarily a work of exogenous criticism, postmodernist thought is neither well elaborated nor critically assessed on its own ground. Postmodernist thinkers are not so much analyzed as judged on the basis of White's interests. As he puts it baldly, he is after an "acceptable approach to ethics and politics." What emerges as acceptable comes out of an attempt to maintain the ontological and moral conceptions that come from his hermeneutic–critical orientation, edified by postmodernist criticism. As a result, his position is never confronted by a strong version of postmodernism, for example, a postmodernist questioning of whether morality and "truth" (even an intersubjectively negotiated version of it) are compatible.

In his attempt to incorporate some of postmodernism's critical postures into an ecumenical synthesis with his Habermasian and Heideggerian interests, White involves himself in an interesting, engaging, and edifying struggle, even though he fails to allow postmodernist thought the space to articulate fully its cautions about such neoenlightenment projects and the different problematics it would substitute.

Many postmodernist positions do not dismiss the idea of a normative ground for action. But they try to exercise "patience" (as Foucault put it) and consistently disclose what is lost by fixing interpretations that permit the legitimation of action. This is not a rejection of legitimation; it is an enactment, rather, of the recognition that legitimation involves a never-ending negotiation that requires the dissolution of certainties. It is thus negotiation all the way down. It is not a joint search for communality or grounds, nor can it count on a transcendent narrative—a temporally unfolding ontology (destiny) that will attract agreement.

However, whatever position one might take on the success of White's attempt at theoretical reconciliation, it does raise the level of debate among Habermasian critical theory, Heideggerian hermeneutics, and postmodernist inquiry. Both those familiar and those unfamiliar with postmodernist thought will find White's book challenging. It is an important contribution to contemporary political theory.

Pauline Marie Rosenau's treatment in *Post-modernism and the Social Sciences* belongs to the more obtuse genre of reaction I have noted. Its scholarship is thin (and often secondary) and its treatment of positions often uninformed and confused. Postmodernist thought, which has a variety of different and challenging implications for the social sciences, is unfortunately often known and characterized on the basis of rumor. One hears that it is a celebration of irrationality, that it is a wholly skeptical and depoliticizing attack on the very possibility of rational political and moral action, that it rejects any conception of history or truth, and so on. Rosenau's book, despite occasional suggestions that there are more reasonable (what she refers to as "affirmative") postmodernists, is a sustained attempt to perpetuate these rumors. Although the advertising blurb on the back of the book portrays the author as neither hostile to, nor apologetic for, postmodernism, the text tends to represent postmodernism as a series of perverse attitudes with very little philosophical depth.

On its face, the chapter-by-chapter list of topics is promising and comprehensive, treating issues of the subject, authorship, history, truth, and a broad range of other epistemological issues central to the social sciences. But, alas, almost every issue is seriously misrepresented. A frequent topic sentence is "Post-modernists argue, . . ." typically followed by a confusing collage of positions that represent the views of no particular thinker and often tend to conflate widely conflicting positions. We are told, for example, that a postmodernist reader has complete license "to attribute meaning to the text without consequence or responsibility" (p. 25). Apart from the fact that most postmodern thinkers are text-oriented, not reader-oriented, it is hard to imagine whose position this might be. We learn also that postmodernism is anti-subject. It is the case, rather, that postmodernist thinkers tend to treat subjectivity as an unstable, interpretive imposition, rather than a fact about consciousness. Accordingly, they often undertake inquiries into the histories of different modes of imposed subjectivity along with analyses of the institutional and other correlates of those modes. Rosenau offers no information or analysis on these undertakings.

Confusions abound. One of the most eggregiously uninformed treatments is that of the issue of "presence" as it is encountered in the writings of Jacques Derrida. Even a slight familiarity with Derrida's texts would make it evident that for Derrida "presence," the idea that the meaning of something is present to the speaker or writer is to be juxtaposed to "representation"; for his argument is that the world of things and relationships is always mediated through representations. The condition of possibility for apprehension is a script—a system of meaning—that precedes and preorganizes any individual's consciousness.

Whatever one might think about the validity of this argument, which is sustained throughout Derrida's early writings, the idea of presence is not to be simply juxtaposed to absence—as Rosenau does, for example, in this vexed passage: "As we have seen, post-modernists argue presence is never absolute: the absent is always present to some degree, and the present is always absent" (p. 79). Adding to the confusion, Rosenau characterizes postmodernists as generally hostile to representation (a position that could be better applied to Marx's attack on the commodity form).

In perhaps her most polemical section, Rosenau states that "almost all postmodernists reject truth even as a goal or ideal because it is the very epitome of modernity." (Here she cites Michel Foucault and Richard Ashley). Apart from the fact that answering the question about what modernity is was central to Foucault's work, this is a puzzling thing to say about a thinker whose historical analyses were organized around discerning the "will to truth." Certainly, Foucault impugned the traditional empiricist view of truth as correspondence (as has, indeed, even empiricist philosophy of science for several decades now) and criticized the hermeneutic view of truth as a recovery of depth, which has supported expressivist theories of meaning. Intensely concerned with truth, Foucault posed the question of how "truths" understood as unchallenged interpretations have emerged and the correlative question about what forms of power they have enabled.

Here, again, Rosenau makes no philosophical defense of a more traditional view. Implying that "truth" (in the form of an apparently unproblematic presence) is both a fact and a moral necessity, she treats postmodernist alternatives as morally offensive. She does attempt to moderate, however, her characterization by again summoning her "affirmative postmodernists," who are less immoral, because they accept some (albeit relative) truths. Here, again, she has missed what is at issue for the thinkers whose work she trivializes. Although it is not the most familiar form of investigation in the social sciences, one of the most significant postmodernist contributions has been its treatment of the historical process by which some interpretations succeed and others fail, contextualized within a concern for both the political implications of the successes and failures and the losses associated with the imposition of historical narratives that gloss over those historical struggles.

In short, readers will not learn very much about postmodernist thought or its implications for the social sciences in Rosenau's treatment; for she has reduced a complex set of philosophical positions, interpretive orientations, and practices of inquiry to simplistic and misleading slogans.

University of Hawaii MICHAEL J. SHAPIRO

What's Left? The Ecole Normale Supérieure and the Right. By Diane Rubenstein. Madison: University of Wisconsin Press, 1990. 215p. $40.00 cloth, $19.95 paper.

This book raises a question that has perplexed political theorists and philosophers interested in the phenomenon of fascism in the twentieth century: How do we explain the complicity of intellectuals with the heinousness of Nazism? Diane Rubenstein's deconstructive reading of the Ecole Normale Supérieure (ENS) cleverly reveals the danger of this question, namely, that posing the question might reveal the rather remarkable fact that a single intellectual discourse and institutional logic may be capable of housing *both* resistors and collaborators. The urgency with which the choice of fascism must be explained, expressed in works such as Sartre's *Anti-Semite and Jew* (1964), Merleau-Ponty's *Sense and Nonsense* (1964), and Simone de Beauvoir's *Force of Circumstance* (1965), is felt still in Robert Soucy's confession that what motivated his writing *Fascist Intellectual: Drieu La Rochelle* (1979) was the nagging question, "How could such a sensitive, intelligent, cultivated intellectual. . . become a fascist?" (quoted p. 76). Rubenstein's strategy in *What's Left* is to turn the question back on the poser, implicitly rewriting it to read, How could someone like me become a fascist?" Her work engages not with the explanatory power of different theories of fascism, but with the power of intellectual discourse to remain blind to what motivates its own efforts to distance itself from mistaken choices by defining them as aberrations or as thought gone wrong: "The 'riddle' of the ENS is *not* how a few rightist *normaliens* can be explained away, but rather, how both right and left can be recuperated within the institutional logic and discourse of The Ecole Normale" (p. 13).

Rubenstein's strategic reversal of the question of fascist intellectuals leads her to resituate "the right within the ENS political tradition" (p. 16). Her decentering of the hold of "leftist stereotypes of the ENS" (p. 12) leads her to "reinscribe writing within politics, ideology within institutions, and at the same time, develop a methodology (or principles of a reading) for the study of intellectuals" (p. 16). Refusing to accept the evidence that because the ENS produced "numerically more famous 'leftist' normaliens" (p. 27), its political affiliation is automatically established, Rubenstein embarks on a systematic (although somewhat tedious) debunking of the representation of the ENS as a left-wing breeding ground.

In Rubenstein's convoluted narrative of the production of the *normalien* from the initial selection of candidates through rigorous examinations to the internalization of an elaborate *normalien* culture (which drew very heavily on elitist symbols, including a pompous language canonized as the signal of belles lettres and worked to create familylike ties among *normaliens*), infamous ENS offspring of the interwar years (e.g., Robert Brasillach, Jean Luchaire, Claude Jamet, and Thierry Maulnier), who penned collaborationist propaganda for a network of journals, move to center stage. Rather than seeing them as exceptional, Rubenstein prefers to make them and their fascism paradigmatic of the ENS sensibility: "(T)here would appear to be a fundamental compatability of notions of cult, athleticism, physical ordeals, eccentricity, dandyism, self-conscious elitism, and distrust for the *moyenne*, evidenced in the *normalien*'s self-perception of its rite of institution. . . with those themes of fascist ideology" (p. 68). That this same school could have produced Jean Paul Sartre, Jean Hyppolite, or Simone Weil becomes, in Rubenstein's reading, what needs explanation.

There is an unarticulated connection between "writing" and the "right" that ought to be taken seriously, Rubenstein contends, not only in order to understand the ease with which fascist support was found among *normaliens* but to appreciate more fully the significance of the postwar trials of French intellectuals such as Brasillach and Jamet, who were accused and ultimately condemned for collaborating with the enemy. These trials animated French intellectuals of the period, leading some writers associated with the resistance to circulate petitions for clemency. Rubenstein's book makes an important contribution to the literature on what Otto Kircheimer called "political trials." But instead of reading these trials only as one way for a successor regime to distance itself from its tainted past by symbolically purging—one easily delineated segment of the population" (p. 158), Rubenstein proposes to interpret the trials "as both a judgment on and protection of an educational system that ritualizes speech" (p. 139): "What made the French purge trials noteworthy . . . was the insistence that words were deeds and should be so tried" (p. 139). These trials of "purely literary collaboration" (p. 138) led to the damning of intellectuals for their words and their signatures. Brassilach and others were condemned to death.

The strength of Rubenstein's interpretation of the ENS as a likely producer of fascist collaborators rests on her articulation of the hidden circuit between politics and writing and her insistence on the "tie between the principles of linguistic and political authority" (p. 14). Ironically, this linkage may exceed the boundaries of Rubenstein's own construction of it. She notes in passing that many of the codes for establishing the *normalien* discourse as an authoritative discourse—linguistic codes such as neologisms, a language of hyperbole, and "poetic devices such as repetition, alliteration, rhyme, etc."—are not wholly unique to this elite" (p. 102). These codes, however, may be more pervasive than Rubenstein intends. Her work draws heavily on "current poststructuralist thought, much of it produced by former students of the ENS: Derrida (promotion 1952), Bourdieu (promotion 1951), Foucault (promotion 1946), and Debray (promotion 1960)" (p. 16). Despite its insistence to the contrary, the discourse of poststructuralism can be read as trafficking in some of the same codes that Rubenstein contends led to the articulation of "notions of hierarchy, difference, and exclusion implicitly, in a manner similar to that of the right" (p. 102). After all, Rubenstein relies on Derrida's and Foucault's own deconstruction of the concepts of agency and subjectivity as disciplinary practices when she concludes that "we should view the author as a victim of his writing which is the social product of an institutional practice—in this case the Ecole Normale" (p. 138). If we can no longer read a text as the property of its author or search for the author's intention as the key to decoding, then the same deconstructivist practice is permissible with regard to poststructuralist texts as well.

Since the political allegiances of one of poststructuralism's most celebrated figures, Paul de Man, have been widely debated, it is strange that he remains unmentioned (except for one reference) throughout Rubenstein's work. Despite the fact that this book is about French intellectuals in the twenties and thirties, the ghost of de Man haunts these pages. The unintended contribution of this densely argued book is to invite us to reverse the question and its focus yet again and ask whether, if we are to see deconstructivist readings and writings as "active interventions . . . that transform contexts" (p. xv), we will not have to ask, What's Left? again.

San Diego State University KATHLEEN B. JONES

Discourses on Society: The Shaping of the Social Science Disciplines. Edited by Peter Wagner, Bjorn Wittrock, and Richard Whitley. Boston: Kluwer Academic, 1991. 386p. $136.00.

In bringing this fifteenth *Sociology of Science* yearbook to a close, the editors claim that the sociology of science is "in a sense the very reflective core of social science." This claim would have made little sense not too many years ago when the sociology of science was just as causal, structural, functional, and nonreflective as sociology itself. The apparent truthfulness of the claim today depends on the sociology of science's having taken a historical turn, made alliances with disciplinary historians and philosophers outside of sociology, and joined the collective effort to undertake detailed investigations of the diverse discourses, practices, and political involvements of flesh-and-blood social scientists in different disciplines, contexts, and times. Thankfully, this sort of reflection is impressively on view in this volume by twelve European and North American scholars in political science, economics, philosophy, and history, as well as sociology.

The 13 essays focus on the shaping of the social science disciplines, particularly in connection with their institutionalization and affiliations to the state. In moving away from history, moral philosophy, and Enlightenment reformism, the social sciences were institutionalized mainly during the late nineteenth century with the formation of different academic associations and theoretical discourses. As a collection, the essays indicate that this process was uneven and differentiated, however, in various national contexts, especially the United States, France, Italy, and Germany. Moreover, the institutionalization was not always at research universities but at training centers for administrative elites (as in French political science) or at commercial colleges and mechanics' institutes (as in English economics). The state played similarly diverse roles in these processes, from actively providing directives about the education of experts to passively playing the constitutive object of grand theorizing. Desrosieres writes, "As the very etymology of the word shows, statistical activity [was even] in league with the State". Yet the intellectual fields established in the late nineteenth century did not generally persist, either in the case of German(-inspired) theories of the state, or even in the case of the so-called classical sociology of Weber, Durkheim, and Pareto. In British political science and Swedish sociology, a clearly articulated social "science" did not emerge until after World War II when "the American model" of behavioral and empirical studies completed its domination of the European imagination. Today's social sciences largely conform to this model and play, according to the editors of the present book, "the correlate of a full-blown modern interventionist welfare state".

The principal casualty of this collective and differentiated portrait, as Anthony Giddens points out in the preface, is the neopositivist view (held by hardly anyone these days anyway) that the social sciences progressively accumulate knowledge in strict autonomy from the rest of social and political life. The methodological consequences of this casualty are not spelled out, unfortunately, save for the editors' fleeting reference to an alternative "critical realism." This philosophical stance, is not shared by all the contributors and would make little if any difference to social scientific practice even if it could be verified.

More engaging methodological issues, however, are raised (or at least displayed) in connection with the historical and sociological interpretations provided by the contributors themselves. Gunnell advocates a "disciplinary archaeology" that peruses the textual monuments of the past and acknowledges only the "congenial ambience" (not the explanatory power) that contexts provide for understanding their meaning. (The editors should have glossed this as a "contextually sensitive textualism" as opposed to a "textually sensitive contextualism"; and much more remains to be done under that label in the case of the history of American political science). Vout would turn our methodological attentions to the "idioms" of social science and its precursors; and he does this, in part, by composing an imaginative "pastiche of inaugural addresses" by Oxbridge chairs of politics between 1920 and 1950. Heilbron rightly calls attention to the importance of "predisciplinary history" for establishing the discourses of incipient disciplines. Desrosieres hails a "social history of cognitive forms" that would connect conceptual schemas to political categories, especially those about, or provided by, the state. Tribe insists that we should understand the public discourse of the social sciences in institutional, not theoretical, terms and proceeds to attend to the syllabi, class rosters, and fiscal endowments of a variety of educational sites (rightly calling attention to economic historians' "sheer ignorance of what went on outside Cambridge and London"). These various methodological strategies are not all in league; but they prove by their substantive fruits the merits of methodological pluralism in the history of the social sciences if not, indeed, in the social sciences themselves.

Given the strengths of the volume, one complaint must be lodged. If, (as the contributors imply) the separation of academic from public discourse that resulted from institutionalization is regrettable, then what, if anything, might repair that relationship, and can we be sure the academic history of social science does not reinforce that very separation?

University of Minnesota JAMES FARR

AMERICAN POLITICS

Politics and Economics in the Eighties. Edited by Alberto Alesina and Geoffrey Carliner. Chicago: Chicago University Press, 1991. 296p. $45.00 cloth, $17.95 paper.

Shifting the Burden: The Struggle over Growth and Corporate Taxation. By Cathie J. Martin. Chicago: University of Chicago Press, 1991. 251p. $43.00 cloth, $15.95 paper.

It would be hard to find two books on American economic policymaking that were more different than these two. Alesina and Carliner's broadly focused book is an edited volume of articles by major figures in the public choice tradition writing on a wide variety of economic policy areas. It is primarily concerned with the question of whether events in the Reagan–Bush period represented a new relation between economics and politics or simply confirmed existing relationships. Cathie Martin's narrowly focused work is a first book by someone in the quasi-Marxist "state politics" tradition, primarily concerned with the underlying changes in political economy since the 1950s that led to alterations in the corporate income tax. The writers in the Alesina and Carliner volume try to trace everything to personal interest and have little room for the role of beliefs. Martin thinks economic ideology is a prime driver of American economic outcomes. Martin looks at qualitative historical data to determine how policy was made. The Alesina and Carliner writers prefer to look at quantitative indicators where possible. The reader is particularly struck by the fact that the literature most cited in each book is largely ignored in the other. Nonetheless, both are useful and interesting studies that widen our knowledge of economic policymaking.

While neither book will take your breath away, both have something to offer. Martin's book is divided into three parts. In the first two chapters she outlines her theory. In the bulk of the book, she examines five case studies of corporate tax bills. In the short final chapter, she talks of implications for the state autonomy and business power literature.

She wishes to make the case that changes in corporate taxation were predominantly due to a series of alliances between the presidency and various parts of business aimed at promoting economic growth. Although she calls this a coalition, she clearly sees business as the junior partner, organized by the executive branch in pursuit of its growth strategies. Because these growth strategies change over time, the executive allies itself with different sections of business at different times, generally choosing the section that gains the most or loses the least from the proposed changes. She identifies four different periods in the postwar era: a neoclassical period, a Fordian–Keynsian period, a hyperaccumulationist (or supply side) period, and a high technology period with different ideologies on growth. She thinks that the executive linked itself with different sections of business in each period and that major changes in corporate taxation resulted.

Having laid out this theory, she then presents a series of five case studies of corporate tax change since 1960. I found these case studies to be well researched, well laid out, and interesting. She made extensive use of material from presidential libraries for the early period and of interviews with participants for the later case studies. The case studies reveal interesting details about splits within administrations and splits between different sections of business. They also provide new material on the organization of business coalitions by the executive branch in pursuit of corporate tax changes. They are, however, somewhat unbalanced. She gives too little attention to what happens in Congress, to Congress's role in initiating legislation, and to coalitions between Congress and business. In the later cases, I also thought that she was sometimes stretching thin material to try and prove her thesis. For example, an executive–business coalition where only 20 of the top five-hundred corporations sign on is not one that overwhelms the reader with its power. However, one cannot carp too much. Precisely this focus on one element of the political spectrum produces most of what is new and fresh in the book.

Do the case studies support the theory? As is so often the case, the answer is *yes* and *no*. She makes a case that both political and economic views on what causes economic growth have changed over time and that these have sometimes affected corporate tax changes, especially in the short term. Her focus on the business–executive relationship also brings into prominence an element of the political process that has not been much emphasized in other literature. She also clearly demonstrates that the executive branch has often tried to organize a supportive business grouping. Thus, what she stresses is true as far as it goes.

But what she omits is as important as what she includes. There is little discussion of distributive motives, the role of parties and party ideology is shortchanged, and there is only cursory consideration of electoral motives and the interaction between changes in corporate taxes and other tax changes. She also downplays the role of Congress as an initiator of policy (especially in 1981) and fails to look seriously at the alliances between Congress and business. And, despite her mention of Ronald King's work on redistributive taxation, she did not look very seriously at the possibility that growth arguments are a way of disguising tax changes that have unpopular distributive consequences. To be fair, many of these points are mentioned in one or another of the case studies; but they are then downplayed in the summaries and theoretical sections.

A few other points bear mentioning. I found her grasp of the literature in the area somewhat selective. There is no mention, for example, of Pierce's earlier study of the Kennedy–Johnson tax changes nor of relevant work by Hibbs, Tufte, Gordon, Buchanan, Sundquist, Page, or Wildavsky. I found the part of her theory arguing that we have had four paradigm shifts in 30 years unconvincing. I read her case studies as showing that business acted mainly in its short-term interests, rather than as part of an alliance. I also have a bias toward testing one's theories against plausible alternatives. There was little attempt to do so in this book. The reader will get from this book the first serious look at the politics of corporate taxation, some interesting case studies, and an attempt to create a theory that is interesting, if not always clear or supported by the data.

The Alesina and Carliner book is an attempt to apply the insights of public choice theory to economic events under the Reagan administration. There is a conscious attempt on the part of most (but not all) of these writers to eschew their usual mathematical models and quantitative tests and to make what they have to say more accessible to the mathematically impaired. There are two obvious questions. One is, What does the book have to say about the political roots of the economic changes under Reagan? The second is, When shorn of the equations, what insights does the public choice perspective give into economic policy making?

The articles in the book deal with most of the major macro policy areas. They discuss political business cycles, monetary policy, tax policy, welfare expenditures, the deficit, trade policy, the savings-and-loan crisis, and minimum wage legislation. Most of the articles contain as part of their substance a brief summary of developments in each area in the 1980s. With the exception of the article on the deficit (which I found misleading), these minihistories are well executed and informative, going beyond mere description to provide insights into the motivations of key policymakers and how external events impinged on them. I. M. Destler's account of trade policy developments is especially well done. The articles also contain excellent summaries of the state of the art in the public choice literature. Anyone wanting to find out what the public choice theorists have been doing in this and related areas in the last five years could not find a better place to start. Fiorina's summary of the political business cycle literature is especially masterful.

But what does this approach teach us that we did not know before? The answer is *not a whole lot*. We learn that presidents do better when the economy is strong; that regulatory agencies get captured by members of congress, who themselves get captured by active constituents; that the vote of members of congress is affected by their party affiliation and their ideology; that when the two parts of Congress are in the hands of different parties, there is more conflict and that this conflict is often resolved by increasing expenditures; and that the president's veto power is important. While these points are developed professionally and applied well to the data, they are not breakthrough insights.

The book is blessed with a number of smaller insights and elaborations on common models that add to our knowledge on the margins. But there are also one or two more provocative insights. My own favorites were the ruminations on the role of the Federal Reserve and the Office of Trade Protection in the articles by Jim Alt and I. M. Destler. The latter has an interesting variant on Kane's (1987) model of regulatory agencies as convenient scapegoats. Alt uses principal–agent theory to combine the somewhat disparate evidence on who really controls monetary policy. I also liked Fiorina's attempt to show that party affiliation was a lagged effect of economic policy. While I was unconvinced by his model specifications and found his evidence less than awe-inspiring, the idea is one that I have always thought had possibilities; and Fiorina makes a good logical case for it.

While these chapters have many virtues, they also have omissions and shortcomings. The most obvious is the excessive concentration on Congress. While Martin gives Congress too little attention, the writers in this book give it too much. Eager to combat the view that Reagan altered policy, this group of congressional specialists want to attribute most of the changes to Congress. Six of the eight papers look at little but Congress, and all downplay the role of the president. Private actors are important only insofar as they affect congressional action and are viewed through a congressional filter. This concentration often produces interesting insights into how Congress deals with economic policy, but it gives a somewhat distorted view of the total policy realm. There is also an excessive tendency to claim that extraordinary policies followed from ordinary political processes. While this might be plausible once, it is less plausible over the entire range of policies.

Finally, while many of the explanations offered were reasonable, others would often have been equally reasonable. Congressional timidity on trade issues could be due to a fear of being labeled unpatriotic when the president is combatting trade antagonists; deficits could be due to changing economic views, rather than a divided Congress; the savings-and-loan crisis could be primarily the result of Reagan's policy and high deficits, rather than congressional capture; and so on.

The two books together show that what one gets out of the process depends significantly on the models one takes into it. The Martin book comes out of traditions that emphasize the state, business power, and ideology; and she finds that corporate taxes are formed by an executive–business alliance that is affected by current growth ideology. Most of those writing in the Alesina and Carliner collection study Congress, and believe in the power of rational self-interest and institutional structure to shape outcomes; and they find that economic policy outcomes are largely determined by congressional structure and the self-interest of members of Congress. Such specialization is both the strength and the weakness of these books.

Overall, these are good books written by intelligent people in a difficult area. While the Alesina and Carliner volume is clearly the stronger of the two books, both are well worth reading, and both add to our knowledge about economic policy and our understanding of the uses and misuses of political models.

California State University, Fullerton PAUL PERETZ

Assessing the President: The Media, Elite Opinion, and Public Support. By Richard A. Brody. Stanford: Stanford University Press, 1991. 198p. $22.50.

It is difficult to break new ground in an area of research as well trod as the study of presidential popularity; yet this book succeeds admirably by offering noteworthy theoretical and empirical insights. Brody provides a worthwhile synthesis of much of his earlier work and perceptive new appraisals of the subject. Especially for people not in the field, his cogent review of the voluminous literature on honeymoons, rally events, time, and the economy is "must" reading.

The central thesis of the book is that Americans judge presidents based on their impressions of news reports about presidential policy successes and failures. According to Brody, the news is not merely a reporting of the details of an event itself. It also involves interpretations of the event by political opinion leaders (or elites). When the significance of an event is appreciated by the public, their evaluations of a president will depend primarily on the description of the event in news stories. When the significance of an event is more ambiguous, its meaning will be supplied by the reactions and interpretations of opinion leaders, which are carried as part of the news. If these interpretations are flattering toward a president, Brody argues that the public's evaluations will be similarly buoyant. If public opinion leaders are less enthralled, so, too, is the public at large. Thus, there are two parts to the theory: the news covers elite opinions and the news and elite opinions shape public opinion.

Brody's news model of presidential approval does not preclude consideration of such factors as the honeymoon and rally events, which have been pivotal in other explanations of the rise and fall of public evaluations. Brody maintains that opinion leaders tend to mute their criticisms of a president early in the term (the honeymoon) and during dramatic international crises that pit the United States against an unrelenting foe (rally events). Brody offers the first systematic comparison of the early period of presidents' first and second terms. The results suggest that while there is a honeymoon plateau of support in the first term, there is none in the second term. Presumably by the second term, opinion leaders are comfortable criticizing a known commodity. Brody also describes elite reactions to various international crises, from the U-2 incident during the Eisenhower administration to the Iran–Contra affair of the Reagan administration. He observes that during some of these crises, when the danger seems remote and presidential folly is apparent, elite criticism, rather than kudos, mounts. In this way, the international crisis (e.g., Iran–Contra) becomes a negative event, rather than a rally event, causing presidential approval to fall.

The core of the book is an intriguing analysis of the news model, offered in two parts: Kennedy through Ford and Carter and Reagan. Unhappily, because of some lost data sets, a combined analysis from Kennedy to Reagan is not presented. The news stories are distinguished between foreign and domestic news; policy proposals and policy results; and good, bad, and neutral stories. The results show that the net difference between good news and bad news affects public approval (relative to the effects of presidential broadcasts and the honeymoon in the first period and to the honeymoon, the economy, past approval, and the hostage taking in Iran in the second).

Even with this ambitious effort, work remains to be done in three areas. First, while Brody clearly outlines the relationship between media coverage and public opinion, he leaves for another book a test of the indirect relationship posited between opinion leaders and public opinion. As currently presented, opinion leadership is only vaguely defined, and opinion leaders constitute a somewhat amorphous group. They are primarily reporters' sources, including the president, vice president, presidential candidates, some members of the White House staff, the congressional leadership, some committee chairs, and columnists. No mention is made of governors, cabinet members, business leaders, or reporters and anchors themselves. In addition, the empirical treatment of elite opinion is largely anecdotal. Future efforts can develop a classification scheme that distinguishes news coverage of opinion leaders' comments about an event from news coverage of the event itself. This would provide a systematic test of the posited effect of elite opinion on news coverage and offer a way of judging whether opinion leadership takes place.

Second, the book does not fully distinguish between presidential opinion leadership and other elites' opinion leadership. In the Kennedy–Ford period, a variable measures presidential broadcasts, but this is not repeated for the Carter–Reagan period. The book acknowledges, but does not study, White House efforts to counter elite criticisms, create elite support, and generate its own favorable publicity. Other projects can build these activities into the news model more fully. Third, as part of presidential opinion leadership, presidents often offer symbolic presentations, as well as the policy proposals and outcomes Brody discusses. Many news events are mixtures of both. It would be important to know how these symbolic components of presidential news affect public approval.

In all, this valuable book offers scholars of the presidency, the media, and public opinion an approach that should be taken seriously and that offers an opportunity for future research.

University of Arizona LYN RAGSDALE

Ballots of Tumult: A Portrait of Volatility in American Voting. By Courtney Brown. Ann Arbor: University of Michigan Press, 1991. 232p. $34.50.

Courtney Brown's book is an ambitious one. He attempts to model electoral volatility in the United States in such a way as to resolve the debate about mobilization versus conversion during the New Deal realignments; develop a general picture of the impact of third parties on the American electoral universe; explain "where all those new female Republican voters came from in 1920"; and build a model of the congressional mobilization cycle incorporating variables derived from a wide range of theories about congressional voting. Brown argues that he addresses these questions from a new perspective, namely, "the possibility of fixed dynamic structures as characteristics of volatile mass electoral behavior differentially related to social context and evident across a wide range of events has not been extensively explored. . ." (p. 11).

Ballots of Tumult contains, in addition to its theoretical chapters, a case study of the dynamics of presidential voting during the New Deal realignment (1928–36); a chapter examining the sources of third-party support (and the subsequent movement of "demobilized" third-party supporters in the instances of the Populist party, Roosevelt in 1912, La Follette in 1924, Wallace in 1968, and Anderson in 1980); a chapter on the 1920 election that examines the differential mobilization of women into the Democratic and Republican parties; and a chapter that attempts to build a new and synthetic model of voting behavior in off-year congressional elections. Brown develops systems of differential equations to investigate each of these problems. For example, in the New Deal chapter, he illuminates (in my view quite successfully) the relative impact of conversion and mobilization by proposing three equations that together constitute an interdependent system describing the over-time fluctuations of three groups in the population: Democrats, Republicans, and nonvoters. Moreover, to communicate the results of his estimations, Brown offers the reader innovative and generally appealing graphic devices (though some are more successful than others).

Brown's is clearly a "bottom-up" perspective on political change. Leaders follow the masses; therefore, it makes sense to work on developing theories of mass behavior in the relative absence of theories of elite behavior. He makes fascinating and cogent points about how organizational dynamics might tie into his model. For example, he speculates that the differential mobilization of women into the parties in 1920 was a function of the local parties' electoral requisites. Nonetheless, he often seems more comfortable talking about mass behavior as a response not to intentional behavior on the part of elites but to "social forces" or "particular social chemistries indigenous to localized milieus" (p. 187).

This said, Brown does take into account critical contextual variables. Not only are his models "conditioned" so that we can see clearly, for example, the relationships between x and y for farm areas versus urban areas, but local levels of two-party competition are central to his view of how electoral dynamics work, and these kinds of variables are built into his models. This seems to me a clear strength of the book. Another is his lucid and useful discussion of the advantages and disadvantages (or pitfalls) of both aggregate voting data and cross-sectional survey data in the analysis of electoral change. He argues persuasively that neither is to be preferred consistently over the other. Finally, independent of the book's argument, Brown has clearly accomplished a monumental and valuable task by integrating diverse data from the ICPSR into a single data set containing county-level electoral and census data.

Ballots of Tumult will serve an important heuristic function, stimulating not only attempts to develop alternative models, but micro-level historical research, as well (which the author suggests is necessary to reach a better understanding of the mobilization of women in 1920). The chapter on the New Deal realignment is perhaps the most clear and convincing. The chapter on the congressional mobilization cycle and that on women in 1920 have a certain ad hoc quality about the explanations; but both, like the book as a whole, raise all sorts of interesting questions. Any reader interested in American voting behavior will find an idea or problem in this book that will encourage the rethinking of a question, the redoing of an analysis, or the revision of a conclusion.

Syracuse University KRISTI ANDERSEN

Catholic Bishops in American Politics. By Timothy A. Byrnes. Princeton: Princeton University Press, 1991. 189p. $29.95.

The Bible and the Ballot Box: Religion and Politics in the 1988 Election. Edited by James L. Guth and John C. Green. Boulder: Westview, 1991. 250p. $34.95.

These two books, one a focused examination of a particular religious elite, the other a collection of essays and research reports about religion in a single election, illustrate well both the strength and weakness of contemporary research on religion in American politics.

In studying the political role of American Catholic bishops, Timothy Byrnes might have emphasized the *process* by which the church hierarchy formulates political policy and strategy. The fascinating story of how the bishops balance their own views with the conflicting imperatives of the Vatican, parish priests and nuns, professional staff members, Catholic interest groups, the various religious orders, and the variegated and increasingly vocal American Catholic community has yet to be fully told. Alternatively, a book devoted to the bishops as political actors might have explored their *impact*, asking how their well-publicized messages on subjects as diverse as nuclear weapons, abortion, and the American economy influenced political elites, ordinary Catholics, and the American public. Eschewing both approaches in this slim book, Byrnes's principal aim is rather to chronicle and explain the emergence of the bishops as a provocative political force in recent American politics.

The recent change in the bishops' leadership style—their increased visibility on a range of controversial issues—was associated with the social transformation of American Catholicism. When they represented an immigrant church whose members suffered poverty, discrimination, and accusations of disloyalty, the bishops' principal political role was local in scope and defensive of Catholic interests and cultural values. When Catholics came out of the ghetto in the mid-twentieth century, the bishops similarly shifted their political role to encompass a broader range of political issues and values, pursue a more national political agenda, and assert a distinctly Catholic political ethic. The increased self-confidence of American Catholics and the enhanced organizational capacity of the bishops—the most common explanations for the new involvement—are only part of the story. According to Byrnes, the bishops simply adjusted to major shifts in American political life—the nationalization of American politics, the politicization of new issues, and efforts by other political elites to capture the Catholic vote. If the bishops became major actors on behalf of the prolife movement in the 1976 and 1980 presidential campaign, that is largely because the Supreme Court transformed abortion into a national issue, numerous candidates attempted to associate themselves with the bishops' agenda, and dealignment loosened the ties binding Catholics to the Democratic party, making them an attractive target for mobilization by conservatives. To those who regard the bishops as unwelcome interlopers trying to impose Catholic values on politics, one might say the bishops were more sinned against than they were sinners.

While the 1988 election may have seemed less overtly religious than its recent predecessors, the essays collected by James Guth and John Green in *The Bible and the Ballot Box* demonstrate a considerably more complex reality. While many observers discounted the role of religion once Jackson and Robertson departed the scene, the studies show a continuing if subterranean religious presence animating elites and ordinary citizens. The chapters following Allen Hertzke's essay—essentially a cultural argument that religion supplied the populist motif in the campaign—examine the political activities of denominational leaders and organizations, the impact of religion on political activists, and surveys of religious influences on the voting of major religious groups—white Protestants, black Protestants, Catholics, and Jews. The collection contains considerable original research, utilizing surveys of Protestant clergy, campaign contributors, and black Americans, as well as elite interviews and secondary analysis of more familiar exit polls and data sources.

In their conclusion, the editors extract an important theme from the diverse studies: "The United States is moving away from the ethnocultural foundations of political alignments, based on conflict between religious traditions, toward a more European pattern, in which conservative parties draw heavily from religious groups and leaders, while parties of the left are supported by more secular forces" (p. 207). As they recognize, this conclusion applies with more force to elites than to masses and does not fit some groups, notably Catholics, black Protestants, and Jews, as well as others. The strongest evidence for it, the disparity in religious commitment between contributors to Republican and Democratic political action committees in chapter 7, is also somewhat suspect. The lower mean score of Democratic contributors on a measure of "traditional religiosity" is potentially an artifact of the relatively large proportion of Jewish contributors in the Democratic camp—a group whose religious orientations are not well represented by an essentially Christian index including questions about biblical literalism and life after death. To appropriate Milton Yinger's dictum, Democratic contributors may be just as religious as their Republican counterparts but religious in a different way.

Both books do a good job of challenging conventional wisdom that portrays religion as a marginal, predictable, and reactionary force in American politics. Byrnes shows, for example, that the Catholic bishops are not a monolithic force but are seriously divided between those who elevate abortion above all other issues and those who regard it as one element in a package of concerns for a "consistent ethic of life." His analysis, along with the studies of religious elites in the Guth and Green volume, demonstrates that the extent of political engagement in any particular campaign depends both on internal factors and the stimulus offered by the political system. Similarly, the studies of groups as diverse as campaign contributors, convention delegates, and voters shows that religion may support a wide variety of political positions and values. After reading these volumes, there is clearly no justification for the all too common tendency to overlook or caricature religion as a political force.

But the two volumes also demonstrate a conspicuous weakness among studies of religion in electoral politics—a tendency to favor description over theorizing. Precisely because of their descriptive nature, these books are rich sources for lecture material and could profitably be incorporated in undergraduate courses. Yet it is hard to say that our efforts to build conceptual understanding of the politics of religion are much advanced by either volume. Byrnes modestly avoids calling his work a theory of elite Catholic mobilization, though there is something approaching theory lurking in his case study. I wonder, for example, whether the same forces he credits with bringing Catholic elites into the political arena might equally well explain the contemporaneous political emergence of fundamentalist Protestant clergy or even some nonreligious groups. A framework such as resource mobilization might make the results of Byrnes's research more interesting to other scholars, and using it could enable Byrnes to offer insights that transcend his case study. Similarly, the excellent studies in the Guth and Green volume tend to stick to the facts rather than connect findings to extant theories about mass political behavior. This is a pity because the studies shed valuable light on such interesting themes as group consciousness and cohesion, social identification, cultural politics, and organizational encapsulation. Unless research on religion is connected explicitly to the theoretical concerns of the larger scholarly community, it is unlikely to influence mainstream research. It would be a supreme irony if scholars of religion in American politics should build a research ghetto just as the subjects of their labor—major religious groups—have emerged to claim a place at the heart of American politics.

University of Florida KENNETH D. WALD

Collective Action and the Civil Rights Movement. By Dennis Chong. Chicago: University of Chicago Press, 1991. 261p. $42.50 cloth, $16.95 paper.

This fascinating book brings together a number of intellectual and research activities that are usually carried on by different kinds of scholars, usually in different forums and without mutual interaction. It would be a mistake for the reader to suppose that this book is a richly detailed, substantive account of the postwar Civil Rights movement in the United States. Rather, it is a successful and needed illustration of how formal theory is developed and used to explain a complex and important phenomenon, in this case, the Civil Rights movement in the United States after World War II. Although the substantive focus is the Civil Rights movement, the author often demonstrates the wider applicability of the approach and analysis to other "public-spirited" movements.

Building on the work of Russell Hardin and Mancur Olson, theorists of collective action have sought to explain the conditions under which utility-maximizing, rational, and self-interested individuals act to produce results that require their voluntary contributions of money, time, effort, and risk to personal well-being. When the object is to achieve justice, fairness, due process, and other public-spirited objectives, the usual problems associated with the efficient production of public goods are accentuated. The special difficulties associated with private provision of public goods like civil rights, women's and gay rights, and even resource preservation and historic preservation are plagued with such routine public goods traits as jointness of supply, nonexcludability, and nonrivalry in consumption.

Dennis Chong formulates a general model of collective action and then applies it to the modern Civil Rights movement. Moreover, he accomplishes this in a manner that should not disaffect those who are uninitiated in the argot of rational choice approaches. Whenever possible, the conceptual apparatus of game theory and public choice are defined in direct, conventional terms, thereby illustrating that there is no necessary reason for formal theory to be surrounded by a notational thicket of symbolic logic or mathematics. Only reflexive fear or hostility should prevent a reading and understanding of the straightforward mathematics and graphical presentation that are sometimes used; in all cases, the verbal explanations are clear and comprehensible.

Considering its conceptual apparatus, it is a relief to find that the text is logically organized. Chong begins with an introduction to his topic, generally phrased as the problem of explaining "public-spirited collective action." The overarching puzzle of collective action is nicely summarized, as are the traditional terms employed to account for it, including public goods, prisoner's dilemma (and its variants, such as an iterated prisoner's dilemma), free-rider, and expressive benefits, among others. The outline of *Collective Action and the Civil Rights Movement* is spelled out clearly and, more importantly, implemented in subsequent chapters.

Chong's analysis is energetically eclectic. For example, early in his analysis, Chong employs the notion of an "all-or-nothing" public good to explain why boycotts and nonviolent demonstrations were effective and sustained in the early days of the postwar Civil Rights movement. He sprinkles the narrative and explanation with references to primary and secondary historical sources and concepts borrowed from the study of social psychology and communications. He ends the second chapter by pointing out that while the persistence of boycotts and protest can be accounted for in game-theoretic and public goods language, the account still does not explain why individuals with every incentive to free-ride actually participate in boycotts and nonviolent demonstrations.

In chapter 3, a variety of "social incentives and reputational concerns" are introduced to answer this question. Friendship, social standing, social networks, and other nonconventional and nonpecuniary incentives are joined with a game-theoretic application of Axelrod's iterated prisoner's dilemma to help explain individual decisions to join and sustain civil rights activity. Chong illustrates how individual behavior and expectations interact in groups so as to create social incentives and reputational concern sufficient to incite and sustain participation in civil rights activity. This chapter is especially interesting, since it focuses on perhaps the core collective choice problem and even provides implicit guidelines on how to mitigate the tendency to free-ride. Chong fails, however, to account for how one can anticipate the conditions under which particular social incentives or concerns for one's reputation will work for any individual. Indeed, there is more than a whiff of post hoc and tautological reasoning in evidence here. After all, concern for one's reputation as a reason for participation is selectively supported by the biographical or repertorial evidence cited by Chong. How to anticipate when such factors do or do not exert influence is not addressed.

The benefits and shortcomings of the overall approach are further illustrated in Chong's treatment of "narrowly rational expressive benefits." These benefits refer to the satisfaction an individual gains from the mere act of doing something, rather than from the putative goal of the participation (e.g., justice, equal opportunity, or abortion rights). A sense of revenge, a feeling of community, and a feeling of fun are presumably folded into calculations of benefits and costs and often produce unexpected levels of participation in public-spirited movements. Finally, with sufficient introduction of nonmaterial, psychic benefits and social-psychological qualifications, Chong gets us to the critical notion of "assurance games," a family of the more general coordination games. A formal model of collective action in pursuit of public-spirited objectives is eventually provided; and only enough mathematics is used to draw out inferences and generate expectations about the impact of various interacting variables on civil rights participation (e.g., the relationship between group demand making and official response, the impact of the schedule of response by officials, and the number of participants in the collective action). Ultimately, the model provides an explanation for the conditions under which participation, success, and, finally, decline in the Civil Rights movement occurred.

In the end, Chong explains that the Civil Rights movement experienced collapse because it was successful in satisfying its participants with its initial agenda and its various "material, social, and psychological benefits." The concluding portion of this book is perhaps least impressive. Chong simply suggests the disappearance of civil rights activism was the consequence of "tired radicals who [were] satisfied with their efforts or disappointed over the impact of their achievements" (p. 239). Saddled with either satisfied or frustrated former activists or cynical contemporaries who are unmotivated by the older schedule of reinforcement, the Civil Rights movement, as it traditionally manifested itself, died.

Yet even though Chong's conclusions about the Civil Rights movement are not startling, he does illustrate the value of rational model approaches to generating a framework for cumulative and replicable social inquiry. The reader should not dismiss this very useful, interesting text for the whimper of its conclusion. The bang here is in the demonstration of how formal and mathematical reason can be employed on behalf of substantive, even qualitative concerns. In this sense, Chong has provided a valuable, successful contribution.

University of California, Riverside MAX NEIMAN

The Postreform Congress. Edited by Roger H. Davidson. New York: St. Martin's, 1991. 354p. $24.00 paper.
Congressional Careers: Contours of Life in the U.S. House of Representatives. By John R. Hibbing. Chapel Hill: University of North Carolina Press, 1991. 213p. $29.95 cloth, $12.95 paper.

Although discussions of the state of congressional research regularly call for fresh data to reassess extant theories and for longitudinal studies that track and explain change, much of what we think we know about the Congress still comes from time-bound snapshots that may no longer be accurate. Congressional scholars need to be concerned with change in the behavior of individual members of the institution and change in the structure and practices of the institution as a whole. A concern with the former drives John Hibbing's careful analysis of congressional careers and a concern with institutional change motivates the essays on the "postreform" Congress that Roger Davidson has drawn together.

Typically, change in Congress has been described in institutional terms, utilizing time series data of the sort reported in *Vital Statistics on the Congress* or in over-time analyses of legislative voting patterns. Hibbing, however, is interested in change at the individual level, specifically during a member's career. He points out that although the literature is filled with findings and assertions about phases of the congressional career and about the impact of increased tenure on the behavior of legislators, the evidence in support of these conclusions is not always very strong. Some are based on little more than anecdotes; others are arrived at either by comparing the behavior of more-senior members with that of less-senior members during the same time period or by comparing cross-sectional data collected at different times. Such research designs can only suggest the change that takes place as members move through their careers and, more importantly, cannot distinguish the change attributable to increased tenure from the "period" effects that impinge on the entire Congress.

To remedy that situation, Hibbing takes a longitudinal approach, developing and analyzing a huge data set covering the full careers of each member of the House of Representatives who entered the body from 1946. As members move through their careers, he looks at five aspects of change: in their electoral situation, in their formal positions within the Congress, in their roll-call voting behavior, in their legislative activity, and in their district activity. In order to test for change across a legislator's career, Hibbing uses standardized measures so that, for example, changes in member involvement in legislative activity attributable to tenure can be distinguished from changes attributable to revised institutional rules and norms governing participation.

These institutional changes have been especially apparent during the last two decades as the Congress moved through the reform era of the 1970s to the postreform era of the 1980s. The Congress of the earlier period was characterized by weak party leadership, strong subcommittees, highly decentralized power, and wide open participation by all members on all issues. This structure, it was argued, well served the needs of members increasingly dependent for their reelection upon their own individual efforts.

The Davidson volume demonstrates that although important elements of the 1970s system remain in place, changes did occur during the 1980s. The scholars contributing to the volume find a resurgence of partisanship in legislative voting behavior (David Rohde), an enhanced role for national political party organizations in congressional elections (Paul Herrnson), a strengthened role for party leaders in the House (Dan Palazzolo), new strategies and opportunities for party leaders (Barbara Sinclair), and new techniques that subcommittees use to defend their proposals in a more democratized House (Barry Weingast). Davidson provides a highly readable historic overview of the emergence of the postreform Congress; and Paul Quirk evaluates congressional performance, a project all too seldom undertaken by congressional scholars.

For the most part, these articles explain change in terms of institutional adaptation to external variables, particularly the altered political environment and policy agenda that arose in the 1980s as a result of changes in voting behavior, divided government, sharper ideological cleavages between the two parties, and higher levels of conflict between the president and the Congress. James Pfiffner, James Thurber, and John Gilmour provide strong discussions of how these changes have affected the manner in which Congress and the president make public policy.

As one would expect, there is some unevenness in the quality of the articles in the Davidson book. They display the wide range of approaches that always has characterized legislative research, from the purely descriptive, even impressionistic, to the more rigorous theoretical and empirical work typical of journal literature. Hibbing's book, in contrast, is a methodological tour de force. He carefully reviews the extant literature on congressional careers, identifies areas of consensus and controversy, and uses his vast data base to reassess much of what we know or think we know about careers.

In the process, some conventional wisdom is reaffirmed. Increased tenure in the House means increased legislative involvement and specialization, lower participation rates on roll calls, a more conservative and somewhat less partisan voting record, and, possibly, somewhat less attention to the district (although the data on this last point are not very strong). But some conventional wisdom also goes by the boards. Increased tenure does not mean greater electoral security. Although formal positions of power come somewhat earlier in the careers of members elected after 1969 than for those elected before, there is little evidence that power is more equitably distributed now than in the past. And Hibbing's data suggest that the widely reported death of the apprenticeship norm may well have been exaggerated. In recent years, the gap between participation rates of new and senior members is greater than it was in earlier years when the apprenticeship norm was supposedly in full flower.

Generally, Hibbing questions the prevailing view of a cyclical congressional career, discovering at several points that recent careers are characterized more by constancy than by change. Increasingly, behavior early in a legislator's career predicts behavior later in the career. The one exception to this generalization—the clear growth in legislative involvement and specialization over the legislative career—leads Hibbing to the conclusion that term limitation would be a bad idea, because it would deprive the Congress of valuable legislative expertise.

Although both of these volumes will find their way into the library of every scholar interested in the Congress, they are also written in a manner that will make them accessible to students and to those in the profession who do not specialize in the Congress literature. The Davidson book will replace a number of reform era anthologies that have long been staples of undergraduate Congress courses and will, I suspect, contribute to the updating of more than a few introductory Congress lectures. But if, as Hibbing argues, there is no longer a life-cycle for legislators, there certainly is one for Congress books; and with the possible exception of the Rohde and Weingast pieces, there are no "classics" in the Davidson collection. Rather, its strength lies in its timeliness; and as times change, the utility of this volume will decline. Because Hibbing takes the long view, because he builds explicitly on the research literature, and because he is meticulously attentive to data and its proper analysis, his book constitutes the more solid and enduring contribution to scholarship on the Congress.

DePaul University MICHAEL L. MEZEY

Why Americans Hate Politics. By E. J. Dionne, Jr. New York: Simon & Schuster, 1991. 430p. $22.95.

America's Constitutional Soul. By Harvey C. Mansfield, Jr. Baltimore: Johns Hopkins University Press, 1991. 236p. $25.95.

Why have Americans lost faith in government? How has American politics gone awry? What has led voter participation in presidential elections to decline steadily since the 1950s and divided government to persist and rendered strong presidential leadership so elusive? And, finally, what is to be done?

E. J. Dionne, Jr. and Harvey C. Mansfield, Jr. address these questions in their books. The failure of both liberalism and interest group pluralism in the 1960s, for both authors, is at the root of the problems confronting Democrats and Republicans in the 1990s. Both also look to a revival of the tradition of civic republicanism in American political thought. But they would revive republicanism in different ways, because they draw different lessons from that tradition for constitutional politics.

Writing for a popular audience, Dionne aims to provide a kind of intellectual history for what has gone wrong in American politics. Voters are frustrated, he argues, because the Left and the Right pose "false choices"—for example, between prochoice and prolife stances on abortion. Americans remain ambivalent about abortion, however. They overwhelmingly favor allowing women the legal right to choose an abortion but deem most abortions morally wrong. Liberal Democrats have lost touch with the values of the middle class, Dionne observes, while conservative Republicans increasingly fail to represent the interests of Americans. Sharply polarized campaigns, sound-bite commercials, and attenuated presidential debates divert us from a serious quest for common ground, or a "new political center" that would revive civil responsibility and build consensus on policies for education, the environment, health care, and other problems facing the middle class and the poor (p. 338).

The liberal consensus that dominated politics from the New Deal coalition, through the Eisenhower administration, to the Great Society programs broke down in the 1960s. That was due to its successes and excesses, as well as from mounting pressure from the Left and the Right. The New Left railed against the "liberal establishment" for not moving far enough toward a more egalitarian, participatory democracy at home and for waging the Vietnam War abroad. Conservatives also distrusted "the establishment," though for different reasons: the establishment was liberal and pushed social welfare policies too far. Notably, Dionne fails to mention that the Warren Court also came together in the 1960s, revolutionizing criminal procedure and greatly expanding civil rights and liberties. Rulings of the Warren Court—and later the Burger Court—reinforced the defection of white middle-class voters, especially in the South, from the Democratic party. Already angry over the passage of the Civil Rights Act, they became increasingly bitter over affirmative action, forced integration, and much else. "Judicial activism," "crime control," busing, and quotas became powerful symbols for Republican presidential candidates from Richard M. Nixon to George Bush. In this regard, the influence of race, civil rights, and economic issues on American politics is handled much more persuasively by Thomas and Mary Edsall in their book *Chain Reaction* (1991).

Dionne follows conventional wisdom in pointing to the decline of political parties and how reforms in the nominating convention of the Democratic party opened up the process at the cost of rendering it the party of special interests. With the breakup of the old New Deal coalition and unable to integrate interest group politics anew, Democratics promised virtually every group something and delivered little, winning only one in the last six presidential elections.

From the failures of liberalism, the second half of *Why Americans Hate Politics* turns to the ascendancy of the conservative Right. After the "fusionist consensus" (p. 161) forged among champions of free-market economics and traditionalists concerned with civic virtue, Dionne surveys the rise of the right wing of the Republican party. Prominent in his story is the neoconservatives' influence and critiques of social programs that went wrong, produced unintended consequencies, and raised social expectations. Republican victories in the 1980s, Mansfield agrees, repudiated much of the 1960s counterculture. Yet they also rested on a precarious coalition of libertarians, religious fundamentalists, neoconservatives, and conservative populists. And both authors note how easily that coalition could come apart.

Government once worked reasonably well. Still, it is little wonder that Americans are losing faith. Both the New Left and the New Right preached distrust of government: "Government is not the solution, it is the problem." The performance of Democrats and Republicans rarely matched their promises. Government succeeded in its failures, rather than building on successful policies. Conflicts over race and civil rights remain, though transformed, from the "cultural wars" of the 1960s and 1980s. They are compounded, moreover, by the further deterioration of communities and inner cities; overcrowded prisons, despite a decades-old battle against crime, tough new laws, a freer hand for law enforcement, stringent federal guidelines for sentencing, and more executions; the excesses of deregulation that led to the savings-and-loan crisis; and a budget deficit that looms ever larger.

Dionne and Mansfield agree on some factors contributing to the problems of governance: the decline of a competitive two-party system and patronage, the pervasive influence of the commercial media and incumbents' preoccupation with campaign contributions, populist appeals, and overly heightened expectations about governmental programs. However, Dionne looks for a new blend of political pragmatism, whereas Mansfield confesses he has "pretty much given up on liberals" and "appeal[s] more directly to conservatives" (pp. ix–x). The problem with liberalism for Mansfield runs deeper than for Dionne. Indeed, the problem lies precisely with pragmatism, participatory democracy, and the abandonment of the formalism and "liberal constitutionalism" of limited government.

America's Constitutional Soul is largely a collection of previously published essays. Part 1 attends to each presidential and congressional election in the 1980s. Mansfield looked toward a possible party realignment and President Ronald Reagan's returning to public discourse respect for the Constitution, while also warning against the New Right's embrace of pragmatism and populism. Pragmatic adjustments nonetheless prevailed; Reagan's legacy is "summed up in the deficit" (p. 62). Part 2 addresses pride and constitutional conservatism, while part 3 deals with religion, separation of powers, and choice and consent in modern constitutionalism. Finally, part 4 defends formalism and "constitutional forms" against the other preoccupations of political science, the mass media's "formless representation" and the informalism of populism and participatory democracy (p. 174).

Americans, according to Mansfield, have been led astray by pragmatism into believing in interpretative choices in their individual and collective self-determination and thus turned away from the "soul" of American constitutionalism: "What America needs is a certain constitutional space between people and government to allow the government to regain control of its affairs, thereby enabling the people to regain control of its government" (p. 45). By contrast, for Dionne, pragmatism and public choice remain the heartbeat of American politics. Scholars already converted to the work of students of Leo Strauss will find Mansfield's arguments useful, particularly in debates over the revival of republicanism. Others may find them, however engaging, less convincing in light of the robust debates waged over rival theories of republicanism (romantic, rhetorical, and antiscientific pragmatism vs. more behavorialist, quasi-scientific versions), as well as over the call for a return to a jurisprudence of original intention. At the same time, Dionne's invocation of the republican tradition and recommendations for reaching compromises on "back-to-basics priorities, [such as] a kind of GI Bill for the 1990s" (p. 348) and vouchers will strike many readers as too easy and conciliatory.

In short, for Dionne, the "body politic" ails because voters confront "false choices," whereas Mansfield maintains that more is at stake: America's "soul is not healthy today" (p. 209) because voters believe they are entitled to choose and, confused about their interests and values, face too many choices. Dionne's solution—more deliberative pragmatism in renewing public life—for Mansfield, only perpetuates problems: what is needed is renewed respect for the formalism of the Constitution, government institutions, and public life. Both books thus merit as much attention for their opposing understandings of the theory and practice of constitutional politics as for their assessments of what is wrong with the political process.

University of Virginia DAVID M. O'BRIEN

Antitrust and the Triumph of Economics: Institutions, Expertise, and Policy Change. By Marc Allen Eisner. Chapel Hill: University of North Carolina Press, 1991. 314p. $34.95.

The first three paragraphs of this book hold out great promise for what is to follow. With a sense for the broad historic vista, the author links antitrust to the foundational principles of the American political economy. He then begins to build his argument, contending that major transformations in governing institutions and alterations in antitrust policy are closely intertwined. Happily, the book fulfills its initial promise. Although the author does not establish connections between changes in antitrust and large-scale political and economic changes that have taken place over the hundred years that antitrust law has existed (as one is led to hope for by the initial discussion), overall, he superbly integrates institutional, organizational, and process perspectives in explaining the evolution of antitrust policy.

The author offers an encapsulated version of his core argument in the introductory chapter and then plays out the various strands in subsequent chapters, rarely losing command of that argument, which is built on the premise that "to understand changes in antitrust, it is essential to examine the way in which the institutions of antitrust have evolved" (p. 15). The engine of policy transformation after World War II has been the "status and organizational presence of economic expertise" (p. 15).

As the author explains, antitrust policy, as embodied in the Sherman Act, was originally ambiguous. It encompassed a host of social, economic, and political goals and was animated by "a liberal apprehension with respect to growing concentration of economic power and its potential effects on local economies and individual autonomy" (p. 228). The ambiguity, reflecting a lack of consensus about what beyond the mere existence of trusts ought to be the object of public policy, placed a substantial burden on the institutions responsible for enforcement—the attorney general and the courts, soon joined by the Federal Trade Commission. Underdeveloped as organizations, the antitrust divisions of the Justice Department and the Federal Trade Commission were largely at the mercy of the courts and Congress in their effort to give more definition and substance to antitrust. The result of court-centered policy refinement and congressional limits placed on agency activism was policy incoherence, and antitrust remained mired in triviality and irrelevance through the New Deal era.

The author argues that building administrative capacity in public organizations requires a conscious effort at professionalization and expansion of a core of expertise. Agencies successful at such an endeavor realize a greater capacity for planning and policy coordination. The price to be paid for this success is that an agency becomes open to, and interconnected with, the external body of experts and other actors concerned with the agency's policy domain.

When the policy domain is characterized by high levels of complexity and is relatively unpoliticized, a special set of subsystem actors employing a common, specialized set of analytical tools tends to emerge, which the author terms a "community of expertise" (p. 92). A community of expertise is "seldom monolithic and static: there are competing groups within the community promoting different visions of policy and administration" (p. 95). Hence, the community and the agencies are vulnerable to changes in doctrine as different groups and their visions rise to prominence.

This is precisely what happened in antitrust policy. Economics provided the logic for a coherent antitrust policy in both the courts and the antitrust agencies, as well as the structure upon which the agencies could build capacity for planning and coordinated implementation. As long as economic structuralism remained the dominant paradigm, moreover, antitrust policy dominated by economics remained consistent with the ambiguous liberal values at its foundation. Through the 1970s, however, Chicago School doctrine replaced economic structuralism in the antitrust community of expertise. The result was the transformation of antitrust policy in the courts and antitrust agencies (the so-called Reagan revolution in antitrust was merely an extension of this change) and the abandonment of the long-standing and still reasonable concerns about market organization and concentration.

I am most impressed with the author's success in tying the internal organizational dynamics of change to changes in the larger institutional and intellectual context of policy, although I believe he understates the extent to which his own case study evidence shows how political appointees provided effective leadership in organizational change.

I am less certain about his contribution to scholarly understanding of how ideas influence policymaking. He provides a challenging discussion; but given his concern for the historical and institutional context underlying antitrust policy and its transformation, it is troubling that he acknowledges only in passing other institutional or even systemic changes to which the antitrust changes may be linked; particularly the rise to prominence of economics in policy analysis and policy decision making generally during the 1960s and 1970s. This led to penetrating analyses of the problematic nature of policymaking dependent on expertise by the likes of Henry Aaron. The author will need to consult these in any further research on the role of ideas and expertise in policymaking.

Eisner also misses the chance to point out that almost precisely the same kind of transformation to a new emphasis on economic analysis in the antitrust agencies occurred at the same time in the Environmental Protection Agency, Occupational Safety and Health Administration, and other regulatory agencies. It would be interesting to know how the author would interpret this more general phenomenon.

These relatively minor shortcomings notwithstanding, I admire this book. It provides rich case study material well placed in a larger historic and political–economic context, no small accomplishment.

Clark University Brian J. Cook

Order Without Law: How Neighbors Settle Disputes. By Robert C. Ellickson. Cambridge: Harvard University Press, 1991. 302p. $39.95.

Order Without Law brings academic discourses on dispute processing, informal systems of social control, state power, property law, rational choice, and the like to an analysis of property disputes involving traditional cattle ranchers, new "ranchette" owners, and local officials in Northern California's Shasta County. Just north of Butte County and not too far from Paradise, Ellickson studied how ranchers resolve disputes over cattle trespass, fencing, and negligence law. This is a multipurpose project. Ellickson wants to test empirically Ronald Coase's "theorem" about how cattle ranchers behave relative to law when their cattle wander onto someone else's property and thus confront theorem with data. He also wants to formulate a general theory of informal order pulled in part from law-and-society research yet modeled by economics and game theory and thus confront data with theorem. In addition, Ellickson wants to build bridges between law and economics and law and society by importing some of the "realism" of the latter and exporting the rational actor model of the former. The book earns considerable respect for imagining such a project and for moving it forward.

Ellickson assembles a picture of dispute resolution in this community, after studying the geography and history of Shasta County, conducting 73 interviews with ranchers and officials, and combing through state statutes, small claims court records, and California Highway Patrol logs. He tells us, "It turns out, perhaps counterintuitively, . . . [that] legal rules hardly ever influence the settlement of cattle-trespass dispute in Shasta County" (p. 40). When "adjoining landowners there decide to split the costs of boundary fences, they typically reach their solutions in total ignorance of their substantive legal rights" (p. 141), and they achieve cooperative outcomes "by developing and enforcing adaptive norms of neighborliness that trump formal legal entitlements" (p. 4). Even when ranchers turn to the law, in cases where cattle wander onto a highway, "most Shasta County residents badly misperceive the substantive law that applies to road accidents" (p. 141).

Do Ellickson's data square with his conclusions? Is law virtually irrelevant in property disputes among ranchers? Ellickson presents a compelling case *only* if we accept an excessively narrow and fundamentally formal view of law. Law *is* virtually irrelevant if in order to find it at work ranchers must know section 841 of the California statute dealing with the allocation of boundary fence costs. However, contrary to Ellickson's conclusions, his data suggest that ranchers do articulate a general principle of "fence cost allocation" that tends to "follow a norm of proportionality" quite similar to section 841 (p. 71). Yet Ellickson insists there is no direct evidence that the statute contributed to the evolution of the norm; thus, he regards "it as more plausible in this instance that norms had influenced law than vice versa" (p. 75).

Impact studies of the 1960s and 1970s also used formal indicators of law. To the surprise of a few, these studies found that the public did not know Supreme Court cases. Lawyers' law does not capture the dynamics of law as a social relation, nor is it an appropriate litmus test when studying popular legal knowledge. What is most interesting about Ellickson's data is not what it says about a knowledge gap between statute books and the experiences of ranchers but what it says about the interplay between property law expressed in texts, like statutes, and property law enacted everyday. Instead of concluding, as Ellickson does, that ranchers "misperceive" their legal risks when their livestock run loose and that they wrongfully worry about liability and the costs of insurance (pp. 104–5), we might reach the opposite conclusion with a more dynamic concept of law. We might study how insurance company–led tort reform in California shaped ranchers understanding of liability. In this type of analysis, law is embedded in, rather than separated from, cultural and political forces that are at work in constituting the everyday social relations of property.

It is against a narrow and formal conception of law that Ellickson portrays the ranching "community" as a nonhierarchical, close-knit, informal group whose norms produce "welfare-maximizing behavior and cooperation without law" in a setting of shared cultural symbols (e.g., living rooms decorated with portraits of John Wayne and rifles). Again, Ellickson's data tell a different story. Rather than confirm his theoretical assertion—"Informal groups are not hierarchies" (p. 232)—we learn that traditional cow-and-calf ranchers in Shasta County are struggling to determine the meaning of property rights with ranchette owners, who are the new retirees and young migrants from the city raising a few farm animals or doing some agriculture on 5 to 40 acres. Stratification among ranchers is not only evidenced in struggles between different classes of ranchers, but Ellickson's data also describes the allocation of status based on whether someone's "word and deed were most unswervingly committed to being good neighbors" or not (p. 211).

Ellickson's treatment of property holders as an "informal group" functioning outside the law is similarly questionable. Not only does the law give them title to the property on which their entire status is constituted for purposes of this study, but the power of property is central to how this group lives. Again, Ellickson's conclusions aside, his description of how traditional ranchers respond to proposals for closed-range ordinances is evidence of the dialectic between property owners and local officials (country supervisors) at work in the constitution of property relations. Near the end of the book, Ellickson suggests that the arrows may in fact go the other way: "Legal policies themselves influence the vitality of informal systems of social control" (p. 284). Readers will be confused by this shift yet perhaps relieved to think that Ellickson is really less tied to the positivist conception of law and the model of causality he embraces earlier (see p. 158).

Ellickson is more successful when he speaks to law and society scholars about the relevance of a "theory of norms" than he is at interpreting his data with this theory in mind. He is on target when he says that a "key shortcoming of the law-and-society school has been its failure to develop a theory of the content of norms. . . . [They] have tended to treat observed norms as exogenous, rather than as dependent variables whose contents are to be explained" (p. 149). Understanding the relationship between rules and norms is a serious empirical and theoretical problem that has motivated law-and-society scholarship for nearly three decades. Ellickson is sensitive to the problem and develops a comprehensive "taxonomy of systems of social control" that serves as a useful heuristic device. Also key to this problem is understanding the relationship between norms and rules by studying the content of both; that is, through an analysis of the content of social ordering, we can better comprehend how boundaries between law and norms are constructed. In places, Ellickson goes beyond his taxonomy, breaks out of his categories separating law and norms, and suggests that indeed the "more ambitious theory would make the content of constitutive rules endogenous" (p. 136, n. 34). For Ellickson, "constitutive norms" are the "informal rules that help glue an informal group together. . . . In contexts where members might not otherwise recognize each other, constitutive rules governing dress, speech, or etiquette may aid identification" (p. 233). While Ellickson speculates that "constitutive and other norms emerge to provide the glue that makes possible a surprising degree of order without law" (p. 236), some law-and-society scholars are now analyzing how law constitutes those norms in particular settings. Studies of the production of legal ideology in ostensibly "informal" settings like mediation question how boundaries between law and norms are constructed by law itself. Again, Ellickson escapes his own boundaries in places, noting that the "law itself explicitly authorizes self-help in many situations" (p. 144). Nonetheless, because of his adherence to a formal conception of law, Ellickson gives little attention to the interpretive processes that invoke and translate legal rules into everyday legal practices.

To law-and-economics scholars and game theorists alike, this book provides a healthy dose of data on the everyday of cattle ranching. Once again, the fiction of zero transaction costs is dispelled. More important for Ellickson, law-and-economics scholars are exposed as "legal centralists"—believing that people bargain from "any initial set of legal entitlements to achieve an identical, and optimal, allocation of resources" (p. 280). It is, however, ironic that while Ellickson seeks to move beyond legal centralism, and even beyond the study of informalism as "bargaining in the shadow of the law," to negotiating "beyond the shadow of the law," he retains a formalist view (p. 40). He marginalizes discursive approaches that might help explain how constitutive norms are produced and circulated in symbolic orders, such as law, because he is unwilling to stray far from the assumptions of the rational actor model, the model that props up legal centralism.

Despite his self-admitted tendency to "exaggerate" law-and-economics and law-and-society perspectives, Ellickson is to be commended for taking on the daunting task of arguing with both camps, serving as translator for each, and in the end seeking to bring them closer together. This book not only examines the debates, it engenders debate. In this sense, *Order without Law* will play a significant role in future debates on the order of property.

New York University CHRISTINE B. HARRINGTON

Political Ambition: Who Decides To Run for Congress. By Linda L. Fowler and Robert D. McClure. New Haven: Yale University Press, 1990. 247p. $25.00.

What decides congressional elections? Why do incumbents do well in most elections, but not in all? Why have the Democrats been able to win majorities in the House of Representatives for 19 consecutive elections, no matter what the national conditions or trends? These are questions that have been central in political debate in Washington among campaign and congressional professionals and among students of congressional elections. While political scientists for decades focused on macro issues of voter attitudes, economic and issue trends, and the impact of presidential elections on congressional races, the roles of recruitment of individual candidates and of political ambition have increasingly come to be a central focus of research.

Early work on political ambition by Joseph Schlesinger and progressive ambition by David Rohde and insights by Samuel Kernell and Gary Jacobson on candidate recruitment in congressional elections have given us a base to build on and spurred further research. *Political Ambition: Who Decides To Run for Congress* is one of the products, and it is a very good one.

While Jacobson and Kernell analyzed broad patterns of candidate recruitment and quality, Linda Fowler and Robert McClure decided on the case study approach. They looked at ambition and recruitment by focusing in detail on one congressional district, New York's Thirtieth District, which, with Barber Conable's retirement in 1984, became one of the most competitive open districts in the country. They believe that by looking at how individual ambition shapes the choices voters have on their ballots and how local traditions and forces shape that ambition by creating or deterring opportunity, we can learn a great deal about how congressional elections work.

Their look at the Thirtieth District extended through both the 1984 and 1986 election cycles—the election of Conable's Republican successor, Fred Eckert, and his defeat two years later by Democrat Louise Slaughter.

By focusing in depth on one district, Fowler and McClure were able to look at all the *potential* candidates for Congress in both parties, including the unseen candidates who never surface in broader press and public speculation because they decide, often tacitly, not to run. In the Thirtieth District in 1984, one of the most interesting and surprising features was that an expected donnybrook over Conable's successor never materialized. Of the more than 30 potential candidates mentioned in the press or by party and community leaders, only 6 formally announced an intention to run, including only 3 who had the recognition and resources to be serious contenders.

In other words, for each formal candidate, 10 serious possibilities opted out. One of them was Louise Slaughter, Democratic member of the state assembly and a top-flight contender, who, for a variety of political and personal reasons, despite the lure of the open seat, decided it was not the right time to run. In 1986, even though it meant taking on an incumbent in the age of incumbency, Slaughter came to a different conclusion and won the seat she continues to hold.

Fowler and McClure carry us through the entire political season in Rochester and its environs, looking at all the possible candidates, placing them in the geographic and political contours of the district, examining their psyches and political and social circumstances. They look at the role of the state legislature and its leaders on the ambitions and decisions of the large number of state lawmakers who considered the congressional race; the role of important interest groups; and the roles of gender, personality, timing, and luck.

Choosing a case to study is risky business; the bill you examine to show how a law is made may never be enacted, and the election contest you study may prove to be no contest at all. Indeed, Conable's seat, when it opened, seemed to be a perfectly sound district to analyze but proved to be anything but routine for the authors:

> In 1984 we followed a low-cost, low-key campaign—without even a primary—that met none of our expectations about a race for an open seat, but still managed to convert a safe Republican district into a highly competitive one. Two years later we saw a modern election involving big money and the mass media, in which a liberal, Democratic woman defeated a right-wing, incumbent Republican in a district with a conservative tradition dating back a century. We could not have invented so many unlikely twists and turns, much less expected them. But this extraordinary chain of events served our purposes exceptionally well, because it drove home the basic premise of our book: results on Election Day depend on decisions made by prospective candidates months or even years beforehand.

The unexpected twists and turns of these two campaigns did, fortuitously, serve Fowler and McClure well. More important, they took full advantage of them to make a good book even more interesting and compelling. With vivid writing, a sophisticated understanding of the district and of politics in both parties and real insights into the motives and perspectives of all the personalities involved, Fowler and McClure have written a gem of a book. There may be no theoretical breakthroughs here, but *Political Ambition* makes a genuine and important contribution to the political science literature on Congress and elections. Scholars of these subjects are not the only ones who can enjoy this work. Everybody interested in politics and elections, from reporters and columnists to lawmakers and campaign professionals, would benefit from reading it.

American Enterprise Institute NORMAN ORNSTEIN

Regulatory Choices: A Perspective on Developments in Energy Policy. Edited by Richard J. Gilbert. Berkeley: University of California Press, 1991. 339p. $39.95.

Reinventing Rationality: The Role of Regulatory Analysis in the Federal Bureaucracy. By Thomas O. McGarity. New York: Cambridge University Press, 1991. 384p. $59.50.

These two books focus on the role of economic analysis of regulation. Significantly, neither book is written by a political scientist McGarity being a law professor and Gilbert an economist. McGarity carefully explores the role that analysis itself (particularly, detailed benefit–cost analysis of particular regulations) has played in recent federal regulatory decisions. Gilbert's book applies microeconomic analysis to energy policy in our largest and perhaps most influential state, California. Both books have implications for the political process of regulation. Both are written well and provide scholars interested in regulation with much useful information, although neither can be described as seminal, pathbreaking, or widely appealing to broader audiences in political science.

McGarity contrasts benefit–cost analysis with other approaches to the development of regulations, presents case studies, summarizes positive and negative elements of benefit–cost analysis, details several structural models used in federal agencies, and considers review of agency analysis by the president's Office of Management and Budget and by the judicial branch. Using case studies from the Environmental Protection Agency, Occupational Safety and Health Administration, Food and Drug Administration, and National Highway Traffic Safety Administration from the 1980s, McGarity compares what he characterizes as the technological or bureaucratic approach to regulatory decision making to the analytic or rational–comprehensive approach that has been increasingly favored by economists on theoretical grounds and by recent presidents, most prominently Ronald Reagan, because it tends to support their policy agenda of minimizing government intervention into private markets.

McGarity includes substantial detail on regulations dealing with leaded gasoline, airborne particulate matter, mechanically processed meat, driver obstructions in automobiles, and workplace hazards. The bureaucratic and interest groups battles reflected in these stories will be fascinating to those who study regulation but might seem tedious to a more general reader. The book provides the reader with an excellent sense of the increasing importance of regulatory analysis as one input into the politics of governmental choice. The detailed stories provide a strong case for the importance of both bureaucratic and presidential staff actors, in contrast to more narrow congressional dominance theories. If the book included a chapter on congressional review of agency analysis, it could be a valuable text for graduate courses in regulation or policy analysis.

The case studies are more interesting than the many typologies or taxonomies McGarity develops to characterize models of agency structure that bring analysis into the decision-making process. To be of greater use, these models should be tied more closely to existing literatures on public administration generally, the role of the policy analysts in government, and the politics of structure, such as Terry Moe's chapter in John Chubb and Paul Peterson's *Can the Government Govern?* (1989). Of the many models he considers, McGarity stresses the superiority of a teamwork approach, an adversarial approach, or a hybrid, as compared to hierarchical or outside advisor approaches.

The balanced treatment of the value and limitations of regulatory analysis is very much like Steven Rhoad's discussion of microeconomic analysis of public policy (*The Economist's View of the World*, 1985). The reader's head nods in agreement when McGarity discusses the positive aspects; but when he next presents the limitations, you wonder why you were so enthusiastic about the value of benefit–cost analysis in the previous chapter.

Gilbert's book applies the microeconomists' useful set of tools to energy regulation. As the product of research by Universitywide Energy Research Group of the University of California, the book covers a broad range of energy policy issues, including electricity rate structures, nuclear power plant economics, natural gas deregulation, alternative energy supplies, the role of supply subsidies, and conservation programs. Chapter authors, oddly, are not identified by profession or affiliation; but they seem to be economists or engineers.

Gilbert does a fairly good job tying the different chapters together to form a coherent book, partly through his role as author or coauthor of 6 of the 10 chapters. Like McGarity, the authors include much substantive detail, illustrated well by many charts, tables, and graphs. The economic analysis is not too complicated and is explained fairly clearly (at the level of a typical Brookings Institution policy book); but political scientists without much microeconomic background may find it difficult. The reader who is not specifically interested in energy policy may find the book too detailed. Still, if the reader wants to know the economics of the various American responses to the "energy crises" of the 1970s, from wind power to conservation to nuclear safety concerns, this book has answers.

The extent to which the book reveals anything about the politics of regulation is overstated. The first sentence states, "This book is an attempt to apply an economist's microscope to the process of public utility regulation." This is misleading, since Gilbert and his coauthors do *not* analyze the process in the way that a political scientist would but, rather, analyze whether the results of regulation meet the criterion of economic efficiency. The politics of the process are not addressed (as they are, e.g., in William Gormley's 1983 classic *The Politics of Public Utility Regulation*). When the authors find that regulation does not measure up to economic efficiency standards (as is usually the case), they then offer some general comments about the likely politics of the issue and why efficiency is not the main goal of politicians and regulators; but the political angle is not emphasized at all.

To someone familiar with microeconomic analysis of public policy, the conclusions of most of the chapters will not come as a great surprise. Typically, the public regulatory intervention has distorted market efficiency and achieved whatever specific goal at a very high cost in efficiency terms. The authors do take great care to test whether the policy choices, like the Diablo Canyon nuclear power plant, were appropriate, given reasonable expectations at the time the choices were made about likely future energy price hikes.

While both books focus on economic analysis, McGarity discusses much more how the analysis influences the politics of regulation and, conversely, how the politics influence the analysis. Gilbert's book treats politics more as a constraint on achieving economic efficiency, rather than something to be studied explicitly. The choice of analytic technique is less controversial in Gilbert's book because it involves comparisons of costs using technologies that have revealed themselves over recent years, while McGarity's discussion of benefit–cost analysis inherently involves the larger unknowns of social regulation's long-term benefits and costs.

Regulatory Choices will be a more useful piece of policy analysis for the regulators (the California Public Utility Commission, other state public utility commissions, the Federal Energy Regulatory Commission, and energy analysts)—, while *Reinventing Rationality* will be more useful to political scientists and policy analysts studying the more general phenomenon of bureaucratic use of analytic information. At its best, the McGarity book can be seen as a parallel to Derthick and Quirk's *Politics of Deregulation* (1985), which focused on how ideas shaped "economic" deregulation. McGarity shows how "social" regulation, too, has been influenced by the increasing penetration of analytic ideas, crystallized in the form of benefit–cost analysis, over the last two decades.

State University of New York, Stony Brook PAUL TESKE

Forging the Military–Industrial Complex: World War II's Battle of the Potomac. By Gregory Hooks. Urbana: University of Illinois Press, 1991. 303p. $39.95.

The Rise of the Gunbelt: The Military Remapping of Industrial America. By Ann Markusen, Peter Hall, Scott Campbell, and Sabina Deitrick. New York: Oxford University Press, 1991. 341p. $35.00.

Why did the military industrial complex develop? What are the effects of this development? These questions are the immediate focus of these two books. Each book also contributes to a more general literature. *Forging the Military–Industrial Complex* contributes to the literature on the theory of the American state, *The Rise of the Gunbelt* to that on the theory of industrial location.

Sociologist Gregory Hooks, whose concern is to describe "the domestic influence of the military in economic governance" (p. 15), traces the origins of the military industrial complex to the New Deal:

> Ultimately the most enduring legacy of the New Deal was not that of reform—it was the legacy of state building, the centralization of power within the executive branch, and the state's growing capacity to shape economic activity. These trends were apparent in the institutions that gave direction to the World War II mobilization—the Roosevelt Presidency, Congress, emergency civilian planning agencies, and military bureaucracies. (p. 83)

Against those who have written that New Deal state development fizzled out in the late 1930s, Hooks argues that during the World War II the New Deal state was converted into the national security state. If anything, the state as a whole was strengthened; but the New Dealers lost out to military bureaucrats and corporate leaders. This happened because "the New Deal depended first and foremost on a secure niche in the federal government and the active support of the president" (p. 123). But the war preoccupied the president and nurtured three agencies—the armed services, the

Reconstruction Finance Corporation, and the FBI—that were hostile to the New Deal (p. 123).

Hooks contends that a major policy instrument used in the mobilization to war was state-sponsored industrial investment, which increased from less than 5% of the nation's capital investment in 1940 to 67% between 1940 and 1943 (p. 127). In the aviation industry, for example, "by 1945, the Reconstruction Finance Corporation held title to 71% of the plants and equipment used to manufacture aircraft and aircraft engines. . . . Only 11% . . . was privately financed" (p. 242).

Finally, although the military industrial complex was born and the New Deal died in the mobilization, both changes were consolidated during the reconversion to peacetime production:

> Reconversion enabled monopoly-sector firms in civilian industries to emerge from the war better insulated from statist planning initiatives and capable of exerting sectoral control. Conversely, the potential for civilian planning by the federal bureaucracy declined as a defense-oriented state sector emerged. . . .
>
> In the process of reconversion, the Pentagon acquired the production facilities at the heart of the military–industrial complex and the administrative authority to operate them in peacetime. (pp. 151, 160)

In short, the engine in Hooks's military–industrial complex theory is the enhanced capacity of the military to govern American industry.

The Rise of the Gun Belt sees the major geographic effects of the military–industrial complex as occurring after the war. After showing that military industry is located far from the industrial heartland, Markusen and her colleagues attempt to explain why. As with Hooks, the explanation for the location of defense industries stresses the "shifting national military mission [and] changing service roles" (p. 5) But it also includes "rapidly evolving technology [and] the adaptive capacities of local entrepreneurs in industry, science, and civic boosterism" (pp. 5–6).

The argument is that World War II involved a change in the military mission of the United States from defense of the domestic shores to a global one. Aviators succeeded in convincing policymakers that World War II proved the superiority of air over land and sea forces. The Air Force became separate from the Army and, in interservice competition, won large shares of the postwar military budget (pp. 16–17). Since Air force doctrine favored "going to industry and having industry develop and produce for us" (p. 32, quoting a general) and the Army had been wedded to developing weapons in the in-house arsenal system and contracting with the automobile industry for fabrication, the military–industrial complex was born.

Which areas won? Basically, a few new centers in areas in which older industries were either absent (as in the South and West) or dying (the Northeast). They were located outside the industrial heartland partly to avoid having to deal with unions and a somewhat antimilitary culture, partly because industry in the heartland had civilian customers and sought to avoid Pentagon regulation, and partly because a new technology was required. The new military requirements were for a limited number of technologically advanced weapons whose construction was extremely labor-intensive. Therefore, the big Midwest-based manufacturing companies with unionized labor forces that had bent metal for World War II weapons were replaced by "seedbeds" of small firms in areas to which highly educated technicians could be attracted. These were often in sunny areas near military bases or engineering schools. The decisions of local boosters (mayors, chambers of commerce), entrepreneurs (founding fathers), and generals and colonels also affected such locational decisions. Case studies of Los Angeles, New England, Seattle, Colorado Springs, and Washington, D.C. show that combinations of these factors were involved in the development of military industries in these areas.

Which areas lost? Mainly the Midwest. For example, about Detroit, which had produced aircraft during the war and had been allied with the army's Huntsville missile production team, Markusen and her colleagues say, "What stands out in this story is the difficulty of a truly distinctive, revolutionary technology taking root in a region where another, entirely different one is still robust and commands resource markets and business culture" (p. 68). Much the same was true of Chicago. Thus, "Until the late 1970s, the major midwestern companies, heavily invested in producer goods and highly successful in consumer durables markets, saw no reason to pursue purely defense outlets" (p. 77). Moreover,

> It was not that the midwest failed to innovate; just that innovation is carried out so differently in commercial and in defense markets. Particularly in consumer goods, the product has to be very simple. . . . This produced a management style inimicable to that favored in defense procurement, where tailor-made esoteric equipment is fashioned with little concern for cost and no presumption of long-term mass production. (pp. 77–78)

Both books address the economic effects of this pattern of military–industrial development. Hooks sees it as a failed industrial policy—misdirected capital investment resulting in de facto national deindustrialization and economic decline. Markusen and her colleagues agree but also see it as a regional development policy with other pros and cons. It did promote economic equality among the regions and stimulated seedbeds of technological innovation although it also created undifferentiated local economies subject to boom–bust cycles and contributed to the urban problems of the current period, including suburbanization and the residential segregation of the poor in declining inner cities. Markusen and her colleagues also say that by attracting middle class migration toward the coasts, it contributed to the political realignment of the country.

These books make several contributions. First, they go a long way toward moving the apparently narrow subjects of American military and military procurement policy toward the center of the study of American politics and political economy. Second, to those who have focused on cross-sectional studies to test parochialism, ideology, deference to the military, and other hypotheses about military policymaking behavior, they offer developmental theories and carefully and subtly interpreted evidence about the historical context to worry about. Third, they challenge researchers in a variety of subfields (e.g., federalism, urban politics, public administration) to investigate historically how the military industrial developments may have affected, and been affected by, their subject matter. Fourth, they, of course, imply a host of questions about how the winding down of the Cold War will affect interservice competition and regional development. Finally, they challenge comparativists to ask whether in other countries the same causes (or their absence) account for military–industrial relations with the predicted societal effects.

Gunbelt is written for a more general audience and would be useful in undergraduate courses, especially if it comes out in a less expensive paperback edition. Both books would do well in graduate courses.

University of Illinois, Chicago BARRY RUNDQUIST

The Logic of Delegation: Congressional Parties and the Appropriations Process. By D. Roderick Kiewiet and Mathew D. McCubbins. Chicago: University of Chicago Press, 1991. 286p. $34.00 cloth, $12.95 paper.

Parties and Leaders in the Postreform House. By David W. Rohde. Chicago: University of Chicago Press, 1991. 232p. $39.95 cloth, $13.95 paper.

After more than 40 years of American political scientists' decrying the decline of party in the U.S. Congress, it is heartening to read two books that point to its resurgence and characterize its importance. The books, *The Logic of Delegation* by D. Roderick Kiewiet and Mathew D. McCubbins and *Parties and Leaders in the Postreform House* by David W. Rohde, reach similar conclusions about the importance of parties in Congress (and principally the House). This is all the more interesting in that they treat entirely different subjects. On the one hand, the Kiewiet and McCubbins book is concerned with whether Congress has abdicated its role in overseeing the appropriations process. On the other hand, the Rohde book is propelled by the empirical observation that in recent years, partisanship and party leadership have increasingly left a mark on the Congress. Both books conclude that party plays a crucial role for shaping behavior in Congress and for providing oversight over important components of the policy process.

There is a good deal that these books share. They begin from a common perspective that has its foundation in contemporary formal models of political actors. This is somewhat unusual, since those ordinarily identified with the rational choice school have given party short shrift. Parties have usually been viewed as temporary amalgams of interest or conveniences used by representatives to minimize information costs for their constituents. By and large, however, political parties have had little status for this group for trying to understand Congress.

Both books share an affinity for providing party a role that is more than descriptive and holds theoretical content. They share a view that political parties are crucial instruments for holding together otherwise atomistic actors. For some time congressional scholars have noted how members of Congress have become insulated from one another and how this has transformed representatives into independent agents who act on their own agendas. The usual caricature is the incumbent—an individual aloof from partisan concerns, a specialist over a narrow policy range, and a delegate tied to the fancy of constituents. What is absent in such a view, however, is the important role played by party in Congress for binding together a majority of incumbents. Kiewiet, McCubbins, and Rohde recognize that party is crucial and begin to offer a theoretical justification for why party is important.

While the clearest statement of a theory of party comes from Kiewiet and McCubbins, a similar view is implicit in parts of Rohde's book, as well. Political parties within the institution are crucial for solving three key problems. The first involves solving coordination problems. In the absence of parties (or leadership, as Rohde argues), even groups of individuals with similar interests can find themselves working at cross purposes. Parties and leaders serve as a focal point around which members of Congress can easily coordinate. Second, parties are key for solving collective action problems. These problems stem from its being in the interest of each representative to pursue a particular policy end (e.g., obtaining a new grant for mass transportation in the district). Yet if all pursue conflicting private ends, all are left worse off (the budget is broken). The key problem here is how to ensure that all bind themselves to agreements not to raid the Treasury. Parties again provide a useful means for binding individuals to collectively desired goals. Finally, the collective choice problem must be solved for legislatures dominated by independent agents who are bound to highly variable districts. In such instances, vote cycling is an all too real problem. While, in recent years, institutional structure has been viewed as one way in which impermanent and cyclic majorities can be thwarted, the authors here suggest that parties also play a crucial role. Parties are important for minimizing voting cycles on the floor through agenda control, constraints on rules, and binding members together. While such observations may not seem earth-shattering, developing a theoretical basis for party in the Congress is key. Then to use those theoretical insights to inform empirical scholarship is impressive and represents a progressive shift for scholars.

Both books point out that party is far more than a label or descriptive attribute that should be included when studying Congress. Instead, party must be understood from a theoretical basis. Both books provide a useful start for stimulating increased theorizing about the nature of parties in Congress.

Rohde's *Parties and Leaders in the Postreform House* begins by surveying the large literature decrying the steady decline of partianship in the House of Representatives. At the same time, Rohde notes that during the 1980s there was an abrupt resurgence of party-based voting. This empirical observation, then, frames the book and raises the important question, "What happened?"

First, Rohde explores the counterintuitive idea that congressional (particularly Democratic) reforms in the 1970s had a delayed effect that contributed to increased partianship in the 1980s. The standard view of those reforms is that they led to increasing decentralization in the House and further undermined already weakened parties. Through a careful blend of theoretical argument, institutional interpretation, and empirical analysis, Rohde shows how the Democratic Caucus was transformed into a useful coordinating mechanism in the 1980s. This was accomplished not just by institutional fiat but with a decline in sectional divisions.

Second, Rohde demonstrates that these reforms strengthened party leadership, with leaders' granted tools that helped them in structuring the legislative agenda. Yet leadership was not given carte blanche. While leaders could shape the agenda, they were bound as agents to the caucus. The combination of enhancing the ability of leadership to lead and binding leaders to an increasingly homogeneous Democratic party contributed to increased partianship.

Rohde is not insensitive to the reaction of Republicans. The 1980s have perhaps best characterized the conflictual nature of divided government, with House Democrats proposing their own agenda to that offered by a Republican President. Rohde is careful to elaborate on the role of Republicans in the House and their response to a more unified majority party. However, the result led to an increasingly frustrated minority cut out of policy formulation. Presidential leadership, he finds, has an interesting effect. While Democrats responded with greater unity in response to Reagan's programs, Carter's own policy agenda tended to fragment the Democratic party.

At the conclusion, Rohde has a very thoughtful, extended discussion of his findings. These are useful in a number of ways. On the one hand, he raises a number of points that question conventional wisdom. In particular, the claim that divided government will inexorably destroy parties does not find much empirical support. On the other hand his speculations give rise to a number of new areas for research. Further exploration of the linkage between electoral responses and the strength of partisanship are certainly in order. So, too, are concern expressed over the nature and strength of leadership.

Kiewiet and McCubbins' *Logic of Delegation* begins by detailing a common view that Congress has abdicated its role of policymaking through delegation. These authors, however, challenge that view, claiming that "it is often the case that desired outcomes can be achieved *only* by delegating authority to others" (p. 3). The received wisdom and Kiewiet and McCubbins' counterclaim are thoroughly subjected to theoretical and empirical scrutiny. The empirical case they detail is the appropriations process. It is an interesting case, since the opportunities for abdication are manifold: the Appropriations Committee has often been portrayed as an independent watchdog of the Treasury, presidential budget making sets important parameters on the appropriations process, and bureaucrats claim specialized information that limits the questioning of their claims. All are sufficient to undermine Congress from generally playing any positive role in the appropriations process.

Kiewiet and McCubbins make an important distinction between abdication and delegation. It is largely accomplished through a theoretical discussion of principal–agent problems. Central to their argument is that political parties are crucial instruments which constrain their agents. To support their point, the authors then dissect a number of institutions in Congress concerned with the appropriations process. Beginning with the role played by party leadership, they then show the power exerted by the party over the Appropriations Committee and committee assignments more generally. Their empirical work certainly undermines the view that the Appropriations Committee acts as a watchdog of the Treasury and clearly points out that both parties have been successful in pursuing their own policy ends. Likewise, their data generally support the point that appointments to Appropriations are not taken lightly and that the parties manage to clone likenesses of themselves on that committee. So, with shifts in party fortunes, we can expect Appropriations to reflect those changes.

After detailing why the Appropriations Committee functions more as a delegate of party than as an independent entity, Kiewiet and McCubbins go on to explore how appropriations bills survive scrutiny on the floor. Certainly, members have every incentive to alter appropriations once such bills come to the floor. Allowing this to happen certainly plays into the hands of those who argue that Congress has abdicated its responsibility. However, Kiewiet and McCubbins contend that party control of Conference Committee appointments provides an ex post veto over amendments. Their theoretical insights here are very useful, and their empirical examples are quite suggestive. Their conclusion seems quite reasonable: the order of the day is delegation, not abdication, since party holds a firm grasp over the final play of the appropriations game. Even when the budget process is removed from Congress, as with the executive and the Office of Management and Budget's initial proposal, Kiewiet and McCubbins demonstrate that congressional parties exert enormous control over what is churned out of Congress.

The most persuasive part of this book lies in chapter 8, where the authors take a careful look at a number of competing theories to see whether party influence stands up. Using some rather elegant econometric estimations of presidential budget requests and actual appropriations, it is quite clear that congressional parties are key to understanding the appropriations process. While the preceding seven chapters are interesting and suggestive, they never cement the argument. Chapter 8, however, ties together the earlier chapters in a very satisfying way.

All in all, the conclusion from this book is quite simple. Congress has not abdicated its responsibility, as too many scholars have argued. Instead, Congress has chosen to delegate authority in particular ways in order to grapple with the contemporary complexity of the appropriations process. While this system of delegation may look rather weak, it is integrally tied to political parties. Kiewiet and McCubbins' argument and findings deserve careful consideration. While the conclusion may seem simple, as they point out, the implications of their argument and findings are quite general and suggestive.

Both *Parties and Leaders in the Postreform House* and *The Logic of Delegation* bear considerable attention from scholars. Both go a long way toward documenting the role of party in Congress. Both rely on important new theoretical considerations about parties in Congress. Both are well-crafted pieces of scholarship. Finally, both will surely pave the way for new research.

Rice University RICK K. WILSON

The Union as It Is: Constitutional Unionism and Sectional Compromise, 1787–1861. By Peter B. Knupfer. Chapel Hill: University of North Carolina Press, 1991. 285p. $29.95.

Too few scholars attempt to draw connections between political culture and politics. It is this attempt that makes Knupfer's book worth reading. The central task of this book is to demonstrate the relationship between political culture and political compromise, or to examine the relationship between civic attitude and constitutional development. While most scholars, especially those focused on legislative relations, look at compromise from the stance of political process, Knupfer presents compromise as a political ideology unto itself.

Instead of being a means to an end, compromise becomes an end unto itself. More than a process of mutual concession, compromise was to be a form of association with distinctive qualities. Compromise is then a metaphor for the union as it was from 1787 to 1861, forming the basis of constitutional theory from the Founders' early vision. By 1861, however, that vision had eroded and was beginning to yield to a new theory.

It is this notion of political compromise transcendent of political process which makes the thesis unique and therefore a contribution to the field. To make his case, Knupfer presents the sectional quarrels of the 1840s and 1850s as a subtext for demonstrating the collapse of compromise as political ideology. The key figure is none other than Henry Clay, the Kentuckian who had served as Speaker of the House. Though Clay is by no means presented as a deep thinker, he is portrayed as a politician who seriously understood the meaning of compromise as the basis for maintaining a constitutional union, which as Madison asserted in *The Federalist* was to be neither wholly federal nor wholly national. Just as Madison, whom Clay admired and worked with, understood that the politics of limited government would flow from a constitutional structure designed to contain power, Clay understood that the politics of compromise arose from the premise that the union was founded on compromise as its own theory; for the most consistent theme of Clay's public life was unionism—fidelity to union. This is what guided his understanding of the constitution and the policies needed to preserve it (p. 123). It is an understanding within the context of a constitutional tradition whose roots are in the early republic. If there is a hero, it is Clay who emerges as the last exponent of compromise.

While Knupfer uses compromise to symbolize the old political order, the rise of political parties, especially the Radical Republicans, comes to symbolize the new order, in which the sectional differences that were tolerated in the past would no longer do. The Compromise of 1850 is then presented as the culmination of a compromise tradition that many had regarded as a successful means of managing sectional conflict: "The political crisis of the 1850s would test a new generation's confidence that parties could save the Union rather than destroy it" (p. 200). Ultimately, then, 1861 is a watershed in American history, since it signifies the death of Madison's union and the birth of Lincoln's.

To this end, Knupfer has succeeded. He has offered us a thesis and supported it through historical scholarship. Nevertheless, some omissions must be noted. While the book begins with an interesting theoretical discussion of the meaning of compromise, one is left to wonder, to what end? What is he trying to show, exactly? If he is trying to add to the general debate as to the Framers' intentions (or original intent, as some might suggest), he nowhere mentions such debate. Knupfer presents compromise as though it was Madison's intention to create a federal system within the context of a liberal framework in which each state could structure itself in accordance with its own political culture. Yet there are those who have suggested that the Framers really had a hidden contempt for federalism, along the lines of the late Martin Diamond. If so, compromise is the result of—not the theory underpinning—the union. It is the process by which the union endures (as Knupfer has correctly noted) and consequently the politics of maintenance. Whether or not the Framers did have contempt for federalism is a debate that may never be resolved, but to omit it does much to undermine the premise that compromise is ideology. Or it may be that compromise was inevitable if the Union was to be at all, in which case compromise could spawn its own culture and brand of politics. That, however, is unclear and remains to be shown.

Nevertheless, the book should be considered for what it is—as attempt to make sense out of a deeply divisive period in American history. And insofar as Knupfer attempts to draw a connection between political culture and politics, he will have made a contribution that is sadly lacking in the political science literature. In this vein, I believe that the strengths of the book do outweigh whatever weaknesses it has. The notion of compromise as political culture and ideology is surely a notion that deserves examination. Insofar as this may add a new dimension to the general debate on the origins of American constitutionalism, the book will have succeeded in making an additional contribution.

Seton Hall University OREN M. LEVIN-WALDMAN

Representation Rights and the Burger Years. By Nancy Maveety. Ann Arbor: University of Michigan Press, 1991. 257p. $34.50.

In this book, Nancy Maveety seeks to accomplish two tasks. First, she argues that in the area of representation rights, "the Burger Court made a unique and distinctive jurisprudential contribution and was not simply an atheoretical interregnum" that followed the Warren Court. Second, she sets forth "the current status of representation law as a result of the Burger Court's work" (p. 221). Demonstrating that the Burger Court undertook the unenviable task of trying to develop a workable case law directed toward the goal of ensuring "fair and effective representation" set forth by the Warren Court in *Reynolds* v. *Sims,* Maveety shows that in so doing it broke away from the simplistic one-person-one-vote standard and propagated what she describes as a "group-balancing approach to representation rights" (p. vii).

By focusing on group-based representation, the Court, Maveety argues, brought constitutional doctrine "into harmony with pluralist politics" (p. 221). The manifestation of this pluralist vision took on several forms, which she orders and explicates in her typology of the diverse groups and images of group representation that the Court recognized. This typology forms the core of her work.

The Burger Court, says Maveety, ultimately recognized five types of representation: geographic (by political subdivisions), economic (e.g., by special district governments), demographic (e.g., racial), partisan (the rights of political parties to police primaries and set membership requirements) and administrative–litigational (access to the courts in order to restrain government agencies). Drawing upon the diverse decisions and opinions in each of the five areas, Maveety demonstrates not only the complexity of the concept of representation but also the Court's difficulties in using the concept as a basis for a coherent case law.

Although the extent to which it can be argued that the Court itself consciously sought to organize its jurisprudence along such lines may be a matter for debate, Maveety's organization of the Court's attempts to cut its way through the thicket of representation serves the vital purpose of bringing order to what appears to be a very chaotic province of the law. As she illustrates, the concept of representing or being represented is quite protean. Not all forms of representation are necessarily inclusive of the interests of the entire society, nor are they intended to be. Accordingly, the establishment of organs for the representation of specific types of interests (such as water district rights) may occur within a society despite the fact that the interests and population of the community involved are distinct and separate from the rest of the society. Furthermore, as the Court argued in *Salyer Land Company* v. *Tulare Lake Basin Water District*, the weight of one's representation in such special apparati may be based justifiably on criteria other than a one-person-one-vote standard.

Maveety's bringing a sense of order to an area of jurisprudence rife with internal contradictions is a welcome and desperately needed addition to our understanding of the Court's attempt to develop a jurisprudence of representation. Instead of exposing a particular resolve within the Court's collective mind, however, it seems that her typology portrays a Court being pulled in several different directions.

One gets the impression that there may, in fact, have been less method to the Court's madness than Maveety contends. In order to sustain her assertion that the Burger Court followed a coherent theoretical path in addressing representation issues, Maveety must subdivide her categorizations several times in order to accommodate and reconcile cases and decisions that do not always complement each other. As a result, the typology is somewhat complex.

This is especially evident in her discussion of partisan representation and the rights of political parties. Despite her primary division of the Court's decisions along the lines of the three functions of the political party (organizational, electoral, and governmental) and the twofold "dualistic historical tradition between a hierarchical party tradition and a populist party tradition" (p. 149), Maveety is still compelled to concede that the model is not comprehensive and that "the justices could be accused of giving primacy to their individual views of what constitutes freedom of association in party operations" (p. 184). Although she shows that the Burger Court's decisions regarding partisan representation were evenly balanced between the two traditions (p. 185), one concludes ultimately that her imposition of an order upon the Court's approach serves more as a means of tracking the Court through a thicket than of excavating a coherent jurisprudence.

This, however, is a reflection more on the Burger Court's inability to grapple with the complexity of representation issues than on Maveety's analysis. Her work is a thorough tour de force of the Burger Court's meanderings in an extremely complex area of constitutional law. It provides an ideal benchmark from which future analysts might embark when attempting to assess the status and development of the Court's conceptualization of representation.

Washington and Lee University MARK E. RUSH

The Closing Door: Conservative Policy and Black Opportunity. By Gary Orfield and Carole Ashkinaze. Chicago: University of Chicago Press, 1991. 254p. $22.50.

In the late 1970s articles began to appear in the nation's press touting Atlanta as a black mecca. The city's growing economic dominance of the entire southeastern United States, its pragmatic embrace of the New South ethic, and the rise of black political power had all helped to attract black in-migrants, many of them from the North, and to encourage the emergence of a significant black middle class. If the barriers to opportunity for the black masses were ever really to fall, it certainly should have happened in Atlanta. But as Gary Orfield and Carole Ashkinaze demonstrate in this passionate book, for most blacks the promise of Atlanta was stillborn.

The Closing Door documents in careful and painful detail not only the relative absence but the deterioration of opportunities open to black Atlantans today; and its authors lay the blame on a decade of conservative politics in Washington and the state capital. Along the way, the book offers reflections—indeed, a sort of test—of various elements of the underclass thesis and of various expectations that accompanied the emergence of black political power at the local level.

Using mainly aggregate data from government records in chapters devoted to income distribution, housing, high schools, college access and performance, and job training, Orfield and Ashkinaze show that things have gotten worse. Rent burdens have increased for poor blacks since the 1970s. Black home ownership rates have declined. Schools are more segregated than ever. Black college matriculation rates are lower, and so on. In part, they blame decisions in the Reagan administration not to enforce civil rights and affirmative action rules vigorously and not to push busing, the dispersion of subsidized housing, or fair housing enforcement. But they also blame the more diffuse conservative assumption that with legal discrimination laid to rest, the market would eventually solve the problem of racial inequality: economic growth and, as William J. Wilson argued, tight labor markets would yield enough work for all and incomes would provide access to better housing, better schools, and better colleges. Finally, Orfield and Ashkinaze blame a misplaced faith in the efficacy of black local political power (though why they consider belief in the benefits of minority political achievement part of the conservative credo is not clear).

What the conservative solution fails to take into account, they argue, is that the color line in the real estate system impedes the operations of the rest of the market. In this book, housing location is everything. Residential segregation is the "fundamental underlying feature of urban racial inequality," and "where a family lives is the single most important factor in the quality of its members' lives" (pp. 12, 29). The black underclass, pinned and isolated in the ghetto—and, indeed, the black lower middle class segregated in suburban enclaves—find it almost as difficult to get a good public education, advance to college, and find meaningful employment as if there were a formal color bar in those institutional settings.

Furthermore, the limits of local political power become painfully obvious, especially in a black central city surrounded by white suburbs. Blacks have controlled Atlanta's city hall since the election of 1973. They control the school district and many city agencies. Black mayors can expand opportunities for blacks in public employment, guarantee a share of city contracts for minority-owned firms, and heighten the sensitivity of public servants, particularly the police, to their minority clientele. But control of city government offers no serious way of gaining access to the suburbs, where employment, educational, and housing opportunities are generated. Nor does city hall have anything to do with education financing or the enforcement of federal civil rights policy.

If neither economic growth nor accession to local office serve to open opportunities, then what is to be done? The authors offer various suggestions, few of them very novel, some of them probably politically impossible, many of them dependent on federal action. A restoration of civil rights enforcement, metropolitan approaches to desegregation of schools and housing, grants for college education, basic skills training, and public employment are among their recommendations. But this is a pessimistic book, and one does not finish it with much sense of possibility.

The book is occasionally repetitious and is full of typographical errors; but these, finally, do not detract from its several important contributions. One of these is the compelling quality of the sheer cumulative impact of the case for declining opportunities, even if much of the material is familiar to students of race relations in the United States. And here and there, amongst the well-established documentation that realtors and lenders discriminate and that schools are segregated, the authors do provide some novel material, such as the account of black Atlanta's trading control of the school system for an end to busing and the data on black access to college.

A second contribution is the book's demonstration that a rising tide does not float all boats. Blacks are worse off in "the city too busy to hate" after a decade of extraordinary economic growth. Market solutions to racial inequality fall short when blacks do not enjoy unimpeded access to the market.

Finally, the book provides a convincing antidote to the charge that the black condition is a product of the failure of the liberal welfare state (as Charles Murray and company would argue). Orfield and Ashkinaze point out that liberal Democrats controlled the national agenda only for a couple of years in the mid-1960s and that after the Great Society, black opportunities widened, and measurable gains were recorded. The deterioration that the authors document sets in under President Reagan's watch and in response to the intellectual agenda of the conservative revolution. It is hard to see in this grim account how it will be undone.

University of Wisconsin, Madison PETER K. EISINGER

The Transformation of the Supreme Court's Agenda: From the New Deal to the Reagan Administration. By Richard L. Pacelle, Jr. Boulder: Westview, 1991. 264p. $39.95.

Deciding to Decide: Agenda Setting in the United States Supreme Court. By H. W. Perry, Jr. Cambridge: Harvard University Press, 1991. 316p. $39.95.

In the last two decades, case selection in appellate courts has been studied with great effectiveness by Larry Baum, Greg Caldeira, and other students of judicial institutions. Since almost all the work on appellate court agendas has appeared as journal articles, however, the two book-length treatments reviewed here are welcome additions to the literature. While both books focus on Supreme Court agendas, the vectors of inquiry are somewhat different. Perry wants to provide a better understanding of how the Supreme Court sets its case agendas and what influences play upon the process. Pacelle's queries have a different accent. Examining 14 policy areas, he asks, What changes have occurred in relative space allocations on the Court's issue agenda over a 56-year period, and what factors have structured the pace and scope of that change?

While Perry focuses on the individual justice as decision maker, Pacelle, following the lead of Thomas Walker, Lee Epstein, and a few others, has characterized his approach as neoinstitutional. This follows from his belief that institutional rules and arrangements "structure the aggregation of individual preferences" (p. 13). Perry suggests, further, that "Cert. votes are primarily not the result of strategic calculations about outcome on the merits" (p. 15). Such calculations are considered "the exception . . . for all of the justices" (p. 276). Again, by contrast, Pacelle sees the "personal values and attitudes [of the justices] and their conception of judicial role" as primary but the impact of neoinstitutional considerations as significant.

In seeking a better understanding of the Court's agenda-building decisions, Perry interviewed 64 of the 161 clerks who served in the 1976–80 terms. He also interviewed five current members of the Court, seven judges of the District of Columbia Circuit Court of Appeals, four solicitors general, four attorneys in the solicitor general's office, and one Court employee. Pacelle, in tracing changes in the Court's issue agenda, uses 7,688 cases decided in the 1933–88 terms.

Perry's volume consists primarily of excerpts from his interviews—information from which he eventually constructs a "process" decision-making model. Other chapters cover such topics as bargaining behavior, internal procedures, and the signals or index of importance to the justices. A useful discussion of jurisdiction is appended.

Pacelle offers an impressive theoretical framework for agenda building and change in his second chapter. Other chapters deal with the transformation of the Court's agenda through relative space allocations to economic, federalism, regulatory, and civil liberty issues. While Pacelle has no concluding chapter, he offers a concluding statement in his last chapter. The book also includes four appendixes, the first three of which define his policy areas, list Supreme Court chief justices by tenure, and catalog Supreme Court cases cited in the book. The fourth is a digest of the important cases used. Both books are extensively footnoted and indexed; and Pacelle also adds a 10-page bibliography.

In spite of Bob Woodward's (1979) *The Brethren* and the circling of the wagons that occurred in the Court subsequently, Perry's work is encouraging. It shows us that the interviewing of Court elites is not impossible and that such interviewing can produce massive amounts of detail in areas in which our knowledge is somewhat general, or perhaps only tentative. Perry's conduct of these interviews also makes the implicit point that best results are obtained when the interviewer "learns" from one interview to the next. Perry has been quite creative in refusing to stick to preproject interview protocols. He frequently shapes later questions to reflect what he has learned in earlier encounters. He is also adept at asking the same question in several different ways during the same interview, (sometimes, it may be noted, gaining a different response). Equally implicit is the significance of "acceptance" by those who are to be interviewed: through his professional contacts and reputation, Perry gained that acceptance. As a consequence, the degree of openness that characterize many of his interviews is quite surprising.

This openness occasionally produces suggestions that conflict with earlier reports in the literature. For example, on the basis of interviews with four justices, Perry reports that the "chief justice begins discussion of the first case on the discuss list" (p. 43) followed by the other justices in order of seniority. He states further that certiorari votes are usually announced at the time of discussion and quotes one justice as confirming the same order for votes on the merits. This is inconsistent with reports by other political scientists relying (as Perry did) on unattributed justice sources.

In the process of preparing this review, I took the liberty of calling this confusion to the attention of Chief Justice Rehnquist. He has now graciously informed me that at present, the justices vote on argued cases beginning with the chief justice and continuing in descending order of seniority to the most junior justice. The discussion and voting is a combined process. Each justice, in turn, first discusses and then votes. When considering petitions for certiorari, the person who put the case on the discuss list goes first, then the vote to grant or deny is taken in order of seniority. This adds more and different information to the picture from an authoritative, attributed source. It should help clear up the confusion to which Perry has directed our attention.

Pacelle's analysis of 56 Court terms reveals that in general, the space allocated to economic, federalism, and regulatory issues declined. At the same time, that allocated to due process, substantive rights, civil liberty, and equality issues increased. While such empirical findings are certainly important, one should not overlook the theoretical and conceptual richness that characterizes Pacelle's work. His efforts in this realm are quite superior to what we normally see in our journals. His ability to start with a general theory and then break it down into components and sketch their interrelationships and ramifications is starkly evident. And once one begins to think he has exhausted the parameters of a theoretical construct, he peels another layer off the onion and stimulates potentially new insights or an occasional feeling of serendipity, though not strictly accidental.

Pacelle's notion of a "bifurcated agenda" for issues and his institutional focus are departures from much earlier work in this area. By devoting his attention to issues, rather than to the decisions of the justices to grant or deny jurisdiction, he directs inquiry to an area about which we know very little. Here he reminds us of issue fluidity and the fact that the decision of the Court to grant jurisdiction is distinct from a decision to confront and resolve any given question. We know from earlier research that issues may metamorphose even in the relatively short time between the jurisdictional decision and case decision. Thus, in the long run, a metamorphosis in agenda space allocations should cause no great surprise. Previous research on the behavior of justices over time has shown that behavior to be dynamic and, in several instances, parabolic. By contrast, Pacelle finds change in issue space allocations to be generally linear. Pacelle's choice of a longitudinal analysis over a static or cross-sectional cut reminds us again that with the exception of Burton Atkins and a few others, we have given insufficient attention to the dynamic processes that characterize the Court and the behavior of its justices.

Pacelle's explanation for the linear patterns he discovers on issue agendas is not extremely radical if stated without adornment. It says simply that institutions—the Court included—reflect their environment. But by conceptualizing the Court as a political entity and suggesting certain changes in the environment as a significant influence on agenda space allocations, he takes a tack that is pregnant with opportunity. Concomitantly, he has left it to others to corral and impose measurement on relevant environmental variables and to determine systematically whether his theoretical notions will bear pineapples or cockleburs.

Finally, it may be noted that the epistemological foundations of these two studies appear to be somewhat disparate. Pacelle appears to accept the very associational or variance models of analysis that Perry finds inadequate and incomplete. Whether the more direct approach of interviewing Court elites is better is certainly subject to question. Almost all the accepted generalizations that characterize our knowledge pool in this area have been produced via the use of quantitative data and some form of statistical analysis. Recognizing that his population of Court elites is not a scientific sample, Perry says, "Much of this book, without apology, is descriptive" (p. 17). But, of course, no apology is necessary, since he offers the book for what it is—a highly qualified and carefully written book—rather than for what one might wish it to be.

Nevertheless, Perry's use of descriptive data permits one to pose a rhetorical question suggested by the comments of two of the justices he questioned. In interviewing his elites, Perry found that the justices were more willing to discuss bargaining and strategic behavior than were the clerks, many of whom, apparently, went into shock when those terms were mentioned. Thus, one of the justices, denying that Warren gained unanimity in *Brown* by force of brute leadership skills, reported that it resulted from a trade of "all deliberate speed" for one or more votes (p. 148). Another said, "I think any one who suggests that this is an objective institution is just wrong; the notion that we are objective is just fallacious" (p. 201). It may be suggested that such a justice would have no qualms about bargaining or strategizing to gain his subjective ends or, at least, that the Court would have no such reservations. This raises the question: Whereas Perry interviewed 64 clerks and 5 justices, would his book have been the same had he interviewed 64 justices and 5 clerks?"

In the final analysis, Perry does not claim to have delivered on his promise to provide a better understanding of how the Court sets its case agendas. He concludes, indeed, that the matter is more complex than he realized and that a broader theory is needed. Yet at bottom, the book has a number of attractive qualities and quite pronounced "discovery" values. It is chockfull of comments by clerks and justices that beg for systematic conceptualization and testing with hard data, if such can be obtained.

To the extent that a book makes a significant contribution to the study of a particular subject, it does so by bringing something new, different, novel, or innovative to a relevant intellectual arena. Another standard is whether the book stimulates one to think in new ways about familiar topics. By either measure, I would give high marks to both the books reviewed here.

University of Kentucky S. SIDNEY ULMER

The Case for Justice: Strengthening Decision Making and Policy in Public Administration. By Gerald M. Pops and Thomas J. Pavlak. San Francisco: Jossey–Bass, 1991. 199p. $26.95.

The authors argue that public administrators understand and implement justice, grounded in morality and practical application, just as implementation of justice strengthens bureaucratic decision making and government policy. Justice, according to the authors' interpretation, incorporates serving the public interest, achieving social equity, and striving for efficiency.

The contribution of this book to existing scholarship is that Pops and Pavlak raise a much-needed agenda of justice in public bureaucracy. This book, for the most part, is descriptive and is not analytical of public administration literature and its varied particulars. The authors never define *justice*, nor do they establish a paradigm for implementing justice in public bureaucracies. The authors offer a paradigm of *administrative injustice* but fail to qualify and/or quantify it.

An antecedent of this work is Frank Marini's *Toward A New Public Administration* (1971). The authors, however, make no attempt to comment on the variety of issues developed in the Marini volume or other relevant literature. For example, the absence of pertinent literature (e.g., Theodore Lowi's interest group liberalism, Arthur Okun's equality and efficiency, Harold Seidman's politics of federal organization, Herbert Simon's decision-making formula, and Aaron Wildavsky's partisan, policy, and systems politics) makes this research quite incomplete. Without references to such literature, any discussion of justice in public bureaucracy is out of context.

This treatment of justice in public bureaucracy is rather esoteric. One presumes, however, that abstractions of justice, by definition, are only intended for, or comprehended by, a relatively narrow audience. In order to attract a more diverse audience, the conceptual material should be interpersed with illustrations and/or case studies.

Despite these flaws, *The Case for Justice* raises an important agenda for partisan–policy–systems politics. The authors do stimulate thinking about administrative justice issues. They argue that a commitment to justice may fill the "legitimacy gap" confronting public administration, stating that justice underlies our constitutional system, that bureaucrats must accept personal responsibility for their discretionary actions, and that advocacy of administrative justice promotes acceptance of change.

Pops and Pavlak examine administrative justice as administrative law with a focus upon the roles of administrative law judges and the Administrative Procedure Act. The authors state that such judges "have shifted increasingly away from rulemaking and an administrative perspective toward adjudication and a judicial perspective [and] have become politically active in representing and promoting their own interests" (pp. 42–43). Meanwhile, the Administrative Procedure Act has helped "to legitimate the role of public administration in American government" (p. 45).

As decidedly more descriptive than analytical, *The Case for Justice* lacks a definitive and consistent methodology. Pops and Pavlak review an array of literature, but they provide virtually no interpretative analysis of illustrations or case studies. Qualities of the just administrator include knowledge and cognitive skills, attitudes, and personal attributes.

The authors elaborate on the multiple justice roles of public administrators. Roles include adjudicator, policy-maker, manager, supervisor of professionals, advocate, public role model, and consumer of goods and services. However, Pops and Pavlak do not illustrate or apply these concepts and roles to particular behaviors of public administrators or case studies.

The most analytical—and valuable—aspects of this text explore types of administrative injustice with pertinent examples. Sources and causal agents of administrative injustice are external and internal, individual and systemic. Type 1 injustices originate from external sources and individualistic causation (e.g., unjust laws); type 2 injustices take origin from external sources and systemic causation (underfunding); type 3 injustices derive from internal sources and individualistic causation (unethical acts of administrators); type 4 injustices result from internal sources and systemic causation (work-flow problems).

Pops and Pavlak end their case for justice on an optimistic note. The crisis of legitimacy in public administration, unethical and illegal conduct by civil servants, and the necessity for expertise, technologies, and resources in public bureaucracies may reform and refocus citizen interest in administrative justice and lead to a more just public sector.

The case for applying justice, for moral and practical reasons, in all public sector activities is a pressing one. Although Pops and Pavlak raise an important agenda, they do not argue very effectively how justice will strengthen decision making and policy in public administration.

Ball State University JOHN E. ROUSE, JR.

The Hollow Hope: Can Courts Bring About Social Change? By Gerald N. Rosenberg. Chicago: University of Chicago Press, 1991. 425p. $29.95.

No one who is seriously concerned about social reform and the role of the courts in the American system can afford to ignore the lessons of this important book. Over the last three decades, *Brown* v. *Board of Education* and *Roe* v. *Wade* have stood at the center of virtually all of our theories, normative and empirical, about the role of the Supreme Court. Social reform litigation more generally has been seen as the hallmark of the modern Court, serving as a lighting rod for both praise and critique. *The Hollow Hope* examines much of the significant social reform litigation of the last three decades, giving particular attention to *Brown* and *Roe*. It persuasively demonstrates that for the most part, the Court has been ineffective in producing "policy change with nationwide impact" (p. 4). The author argues that as a result, groups seeking significant social reform foolishly squander their resources by turning to litigation and, by implication, that in spinning arguments both for and against a Court that serves the interests of political minorities, we have foolishly assumed that the Court has an important independent role in shaping social reform. If Rosenberg is right, and it is very difficult to leave the text unpersuaded, one has to question why scholars, politicians, and reformers attribute as much influence to the Court as they do.

Rosenberg begins with the question, "When and under what conditions will U.S. courts be effective producers of significant social reform?" (p. 9). He juxtaposes two prototypical views of the role of the Court in producing social change, extracting from them an analytical framework for systematically understanding when and why the Court can succeed in producing social reform. Specifically, Rosenberg posits three familiar constraints on the Court that impede the success of social reform litigation: (1) the limited nature of constitutional rights, (2) the lack of judicial independence, and (3) the judiciary's lack of powers of implementation. These constraints can only be overcome when three corresponding conditions are met: (1) ample legal precedent for change, (2) support for change from substantial numbers in Congress and from the executive, and (3) support from some citizens (or at least low levels of opposition from all citizens), with one of the following: (a) incentives for compliance, (b) costs to noncompliance, (c) implementation left to the private market, or (d) individuals crucial for implementation seeking either political leverage or cover (pp. 35–36).

Rosenberg compiles a formidable array of empirical evidence to assess both the direct and indirect effects of social reform litigation. For example, in assessing the influence of *Brown*, Rosenberg not only demonstrates that by and large integration of the schools occurred as a result of congressional and executive branch action, not Court action, he also demonstrates that it was *not* the Court's decision in *Brown* that placed civil rights on the political agenda, precipitated congressional and executive branch action on civil rights, favorably changed the views of white Americans, or served as a stimulus for the Civil Rights movement of the late 1950s and early 1960s. The same thoroughgoing analysis is applied to *Roe* v. *Wade* and various other women's rights cases. A brief overview of how the constraints-and-conditions model can be used to explain the successes and failures of environmental, reapportionment, and criminal procedure litigation is presented in the final section of the book. Rosenberg concludes that "given the constraints and conditions, . . . U.S. courts can *almost never* be effective producers of significant social reform" (p. 338).

The power of this book comes, in part, from the ease with which we can accept Rosenberg's specification of constraints and conditions; they have been drawn from a rich heritage of public law scholarship. What is new is the way in which he synthesizes the common wisdom into a coherent analytical model supported by an impressive compilation of empirical evidence. While every reader will undoubtedly have a quibble with some specific piece of empirical evidence relied on, the totality of the case Rosenberg makes is overwhelming and compelling. This is not a polemic; it is first-rate scholarship.

The combination of Rosenberg's specification of constraints and conditions and his empirical attempts to disentangle the effect of the Court from the effects of the other branches sometimes creates the appearance of a heads-I-win-tails-you-lose scenario. Political support is one of the three conditions necessary for Court-ordered social reform to be effective; in practice, when Rosenberg finds such support, it is sometimes used as evidence that the change would have occurred without the Court's intervention, especially in regards to abortion reform. Given the way the evidence actually plays out, however, this is not as problematic as it may seem at first. Indeed, one of Rosenberg's most intriguing findings is that predecision elite support often evaporates once the Court issues its decision. Similarly, in examining the indirect effects of social reform litigation, Rosenberg intimates several times that while the Court's decisions did not serve to mobilize support for social reform, it did seem to mobilize opposition, although the latter observation is not subjected to the same careful empirical testing that the former is. Repeatedly, the book suggests that not only does social reform litigation unwisely divert group resources away from more productive strategies but that the Court's decisions themselves at times have a counterproductive effect.

As the title intimates, many will find *The Hollow Hope* disheartening (as one suspects the author himself did). They may find some solace in the thought that Rosenberg may be asking too much of the Court by asking that the Court go it alone. His empirical analysis is directed at assessing how much social reform the Court is able to bring about independently; but acting alone, neither Congress nor the executive branch can bring about much social reform, either. Those branches each have their own sets of constraints and conditions. The unasked question is whether the Court, in concert with one of the two branches can bring about social reform (either directly or indirectly) without the support of the other—or in concert with the states, without either of the branches. While it is clear that social reformers who rely solely on the Court for significant reform are bound to fail, that does not necessarily mean that they will not find it useful to employ litigation strategies in conjunction with explicitly political strategies, something few of the reformers involved in the litigation Rosenberg examines attempted to do. Further solace may be gained from remembering that Rosenberg's analysis is limited to "litigation attempting to change the functioning of a whole set of bureaucracies or institutions nationwide" (p. 4). Such aberrational litigation is far from being the whole of the Court's participation in the process of policy development.

This is a tremendously important book. In profound and persuasive ways, it challenges many of the assumptions that undergird the Supreme Court scholarship of the last three decades. *The Hollow Hope* will fundamentally reshape how we see the courts and what questions we ask about them.

Rutgers University SUSAN E. LAWRENCE

The American Political Nation, 1838–1893. By Joel H. Silbey. Stanford: Stanford University Press, 1991. 348p. $35.00.

Imagine a society in which parties dominate the political system. They select the candidates, articulate distinct ideologies, oversee the conduct of elections, virtually monopolize political communications, mobilize voters, organize legislatures, structure roll-call votes, and (more generally) determine the shape of public policy. Perceptibly connected to the daily experiences of most citizens, these entities function, in other words, as political science texts claim parties should.

For those of us familiar with the parties of the 1990s, imagining such a political order strains our creative capacities. Yet only about a century ago, the United States was such a society. And Joel Silbey's book brilliantly captures and recreates that political world.

The American Political Nation, 1838–1893 synthesizes an incredibly diverse array of materials, primary as well as secondary. In the process, it offers an analysis of the genesis of parties as organizations; of their slow, sometimes halting, but eventual acceptance by the mass public; and of the dominant roles they played in campaigns, elections, and governing. There are chapters devoted to each of these topics, as well as treatments of the patterns of popular voting, the ethnic–religious and socioeconomic bases of partisan loyalties, and of those who challenged the reigning partisan norms. Each of these chapters offers a cogently argued, detailed, and richly textured picture of a bygone world in which citizens were avowedly, uncompromisingly, and proudly partisan. It was also a world marked (not coincidentally, Silbey argues) by high rates of turnout on election day, by the enthusiasm of citizens for politics and political activities, and by the capacity of elected officeholders to respond to grassroots impulses.

Silbey's analysis contributes materially to understanding nineteenth-century politics. Along two distinct dimensions it provides a comprehensive picture of that lost world. First, Silbey's view is cross-sectionally comprehensive. It treats all types of political activity—governing, as well as electioneering. Thus, he shows how parties bridged the gap—how voting became related to public policy.

Second, Silbey's view is longitudinally comprehensive. While most studies of nineteenth-century politics focus on what distinguished the second and third electoral–party systems from each other, Silbey, emphasizing what they shared, treats them together as a distinct *political era*. The spread of sectional norms during the 1850s led to the breakup of one major party, the creation of another, shattered electoral coalitions, and culminated in civil war. Yet through it all, "the central reality of partisanly defined and shaped political activities remained" (p. 139).

More generally, Silbey offers the concept of a political era as something of an antidote to the dominant party–electoral system framework. This new perspective, he argues, allows the development of a classification scheme based on political attitudes and actions other than popular voting. The outline of that scheme he offers as a supplement to the critical–election model (pp. 6–8). This aspect of Silbey's argument is less persuasive than most others. To justify his new taxonomy, he construes the party–electoral system framework too narrowly. In Walter Dean Burnham's analysis, for example, it is as broad and inclusive—as capable of dealing with the complex web, or relationships and interactions among voters, officeholders, and political institutions—as the notion of a political era. Moreover, Silbey's rubric really does not seem to designate anything more than others have included under the label *political culture*.

There is another soft spot in the book: too much of its evidence and too many of its examples (especially the great political one-liners) are drawn from the years prior to 1870.

Those who have specialized in looking at shorter time slices and/or smaller geographies, no doubt, may find other reasons to quarrel with some specifics. That is inevitable with syntheses of this sort. But the fact that this book will raise questions, evoke intellectual disputes, and stimulate new research is simply another sign of its significance and importance.

Northern Illinois University PAUL KLEPPNER

Eisenhower and the Management of Prosperity. By John W. Sloan. Lawrence: University of Kansas Press, 1991. 192p. $25.00.

Was the real Dwight David Eisenhower the complacent, reluctant president described by intellectuals in the 1950s or the savvy "hidden hand" president portrayed by revisionists in recent years? The truth is somewhere in between, concludes John Sloan in his analysis of economic policy making in the Eisenhower administration. Far from complacent, Eisenhower emerges in Sloan's book as a conservative activist, deeply involved in domestic policy. Yet contrary to the revisionists, Sloan argues that Eisenhower's political effectiveness dropped precipitously during his second term, when he was less likely to employ hidden hand tactics or effectively balance righteous zeal with political finesse. And despite the revitalization of his image as a leader, presidential aspirants still do not evoke the memory of Eisenhower as they have those of Roosevelt, Truman, and Kennedy.

Drawing upon the resources at the Eisenhower Library in Abilene, Kansas, as well as a wealth of secondary sources, Sloan sought to fill a gap in the revisionist literature concerning Eisenhower's economic policy. He adopts Graham Allison's bureaucratic politics approach to analyze how economic policies were a product of accommodation among various actors, their interests, and their relative influence. The bureaucratic politics approach is more commonly applied to foreign policy decision making; but it is quite helpful in analyzing various economic decisions during the Eisenhower administration. Because of the more formal, hierarchical organization of the administration, the bureaucratic politics approach probably works better for the Eisenhower White House than it would for other presidencies.

Sloan sets the stage for his analysis with a review of the economic philosophy of the administration, the key players involved, and the president's economic advisory system. Brief biographical profiles of advisors such as George Humphrey and Arthur Burns and key decision makers such as William McChesney Martin are offered. The political economy of the 1950s is also considered: the state of the economy, the Cold War, and the partisan context. As frequently occurs with a bureaucratic politics emphasis, however, consideration of external factors is relatively thin. While some of the relevant elements of the political and economic environment are reviewed, the analysis would have benefited from greater breadth and depth of coverage.

Sloan develops evidence to support his assertion that Eisenhower was more of a "conviction politician" than is often assumed, actively pursuing firmly held economic ideas. One of those convictions was to balance the federal budget, which he accomplished three times during his administration, a remarkable feat viewed from the 1990s. However, Ike and the Republicans paid a price for his obeisance to the balanced budget in the 1958 and 1960 elections. The main thrust of the analysis in the book is contained in separate chapters dealing with the budget, monetary policy and anti-inflationary efforts, and fiscal responses to the recessions of 1953–54 and 1957–58.

One of the more interesting sections of the book is a reevaluation of the battle of the budget for fiscal year 1958. This episode has been enshrined in the presidency literature through Richard Neustadt's widely read *Presidential Power* (1960). Neustadt disparages Eisenhower's amateurish political skills because he allowed Secretary of the Treasury George Humphrey to undercut his budget on the very day it was released. Sloan's analysis of the record reveals that not only was the president aware of Humphrey's proposal for further cuts in spending but actively worked with his advisors to determine exactly how the revised proposal would be presented. After editing Humphrey's original four page memo suggesting further cuts, Eisenhower decided that Humphrey should hold a press conference, duly noting his support for the president's budget message. Unfortunately for Eisenhower, the media coverage focused exclusively on Humphrey's extemporaneous comment about a possible "depression that will curl your hair." Sloan shows that there was much more involved in this episode than is indicated by Neustadt, although Eisenhower's political wisdom in allowing the press conference in the first place remains suspect.

Eisenhower and the Management of Prosperity makes an important contribution to the scholarly literature on the presidency, economic policymaking, and in particular to the study of the Eisenhower era. By focusing on the different aspects of economic policy, he provides a sense of how presidents balance competing objectives and interests, use their advisory systems, and respond to external developments. Because the administration portrait presented in the book is necessarily incomplete, this work is most likely to be useful in conjunction with other recent works, particularly Greenstein's *The Hidden Hand Presidency* (1982); it provides a policy focus that supplements biographies such as Steven Ambrose's *Eisenhower: The President* (1984); and it reveals Eisenhower to be less Keynesian in outlook than is suggested by Herbert Stein in *The Fiscal Revolution in America* (1969).

Despite the book's many merits, the contribution to the field is less than it could have been. The book simply is too brief and condensed to do justice to the topic. Much more should have been added about many of the topics covered, both in terms of original data and analysis. The author briefly quotes, but rarely presents, original sources. On many occasions, reproducing the actual notes, memos, or correspondence would have helped make the case much more convincingly and created more of a feel for the Eisenhower White House. The separate chapters treating the budget, monetary policy, and the antirecession measures are not as well integrated in terms of process and outcomes as they could have been. The writing is stiff, sometimes ponderous, and occasionally hard to follow.

While we could have learned more along the way, Sloan's conclusions end the book on a strong note. Eisenhower clearly was not "above politics"; he was more ideological than popularly perceived and occasionally dogmatic about issues such as a balanced budget. He turned away from the more liberal aspects of "modern Republicanism" and towards fiscal orthodoxy as his term progressed. Despite his relative success in the management of prosperity during the 1950s, both he and his critics were dissatisfied by the end of his term. As Sloan concludes, "By the end of the 1950s, Eisenhower's presidency seemed to lack the required vision, and he left office with feelings of rejection" (p. 162). Despite some shortcomings, John Sloan's book does a good job in showing when and how the president's vision faltered, diminishing his political effectiveness and policy achievements.

University of Missouri, St. Louis LANCE T. LELOUP

The Pro-Choice Movement: Organization and Activism in the Abortion Conflict. By Suzanne Staggenborg. New York: Oxford University Press, 1991. 229p. $27.50.

Following the Supreme Court's 1989 *Webster* v. *Reproductive Health Services* decision, journalists spoke of a rebirth of prochoice activism and renewal of energy for moribund prochoice organizations. This view comported with the conventional scholarly wisdom that prochoice groups languished after *Roe* v. *Wade*, while antiabortion forces mobilized. Suzanne Staggenborg challenges this perspective, arguing that the prochoice movement remained viable in the post-*Roe* era by developing formal organizational structures, hiring professional leaders, and employing a conventional political strategy. Becoming an insider, however, did not stop the movement from continuing grassroots organizing. Institutionalization and ongoing grassroots activism prepared prochoice forces for *Webster*. Furthermore, Staggenborg notes that prochoice mobilization following *Webster* was not unprecedented; the Hyde Amendment of 1976 (limiting use of federal Medicaid monies for abortion) also stimulated organizing.

Staggenborg examines newsletters, correspondence, and other documentary evidence of 13 (6 national and 7 Chicago-based) prochoice organizations. Her analysis includes representatives of the population organizations, single-issue abortion organizations, and women's organizations (e.g., Zero Population Growth, the National Abortion Rights Action League, and the National Organization of Women, respectively). Interviews with 50 activists supplement documentary evidence. *The Pro-Choice Movement* provides the most richly detailed and nuanced narrative of the strategies and tactics of prochoice organizations available. It also is one of the first works written by a scholar (rather than an activist, journalist, or freelance writer) chronicling activities of the prochoice movement. Specialists in abortion politics will appreciate the first two appendixes; one presents a brief history of each organization and the other discusses the location and accessibility of the documents used.

The Pro-Choice Movement is organized chronologically into four sections. Part 1 discusses the origins of the prochoice movement from the early 1960s to 1973. Staggenborg contends that the movement originated from within established institutions, as well as through outsider challenger groups (e.g., the National Organization of Women). Seasoned activists who had worked on reform proposals formed new organizations devoted to abortion repeal and were later joined by the burgeoning women's liberation and population control movements. Staggenborg's research expands that of Kristin Luker (*Abortion and the Politics of Motherhood*, 1984) and Raymond Tatalovich and Byron W. Daynes (*The Politics of Abortion*, 1981), who view the transition from reform to repeal largely as a response to the nascent women's movement and to a redefinition of abortion as a moral argument (controlled by women), instead of a medical issue (controlled by physicians).

From 1973 to 1976, intense countermobilization kept the movement alive, as did activism within the women's movement. National prochoice leadership became more professionalized, organizational structures more formalized. After *Roe*, support solidified among such inside organizations as the American Civil Liberties Union and Planned Parenthood (groups now synonymous with the prochoice label). Transformation, rather than demobilization, is the appropriate way to classify this period.

Paradoxically, the prochoice movement gained strength between 1977 and 1983 following passage of the Hyde Amendment (the first prolife victory). The threat to abortion rights represented by the passage of Hyde created additional resources that accelerated organizational trends toward professional leaders and formalized structures. Prolife organizations, however, dictated the terms on which the abortion battle would be fought. In response, prochoice groups emphasized grassroots organizing, increased their use of institutionalized tactics (legislative lobbying, party politics), and adopted a single-issue political focus. Prochoice organizations that advocated multiple-issue agendas (notably the Reproductive Rights National Network) met with little success. Unlike other scholars, Staggenborg contends that growing professionalization did not lead to the demise of grassroots organizing.

In 1983 prochoice victories in the Supreme Court and Congress quashed immediate threats to legal abortion. While many prochoice constituents considered the battle over, direct action tactics by prolife activists coupled with antiabortion activities of the Reagan and Bush administrations kept the prochoice movement alive and prepared for *Webster*. While Staggenborg notes that prochoice organizations are employing increasingly sophisticated strategies and tactics that may not require grassroots activists, they still are promoting grassroots efforts. If these organizations remain resource-rich, conflict between professional and grassroots organizing need not occur.

As a political scientist, I find several weaknesses in the book. Staggenborg writes of triumphs and difficulties in organizing the National Abortion Rights Action League of Illinois, yet the legislature remained staunchly antiabortion after *Roe*. What effect did Springfield's hostile climate have on organizing? Staggenborg's findings may have limited generalizability because her cases do not include variations in state political context. Few references to relevant political science scholarship appear; noticeably absent in Jane Mansbridge's *Why We Lost the ERA* (1986). Last, the book's theme is that mobilization begets countermobilization, an analysis that is easily subsumed within Trumanesque pluralism.

Staggenborg's principal theoretical contribution is to the study of social movements. Social movements that survive the "dry" periods of mobilization generally do so by establishing formalized structures and professionalized leadership (or its equivalent). Her first-rate, theoretically informed history of the prochoice movement is a much-needed addition to the abortion politics literature. While not as lucidly written or as powerfully argued as Luker's work, *The Pro-Choice Movement* ought to be read by students of women's politics, abortion politics, interest groups, and social movements.

Furman University GLEN A. HALVA-NEUBAUER

Public Service Liberalism: Telecommunications and Transitions in Public Policy. By Alan Stone. Princeton: Princeton University Press, 1991. 296p. $35.00.

In renascent conservative ideology, government "is the problem, not the solution." Since the 1970s, this message found receptive ears in a populace disillusioned both by government's social programs and the performance of the economy. Celebrating the miracle of Adam Smith's invisible hand, the virtues of free markets, free enterprise, and laissez-faire, policy designs advocating privatization, deregulation, and competition have been formulated by the policymaking communities in the United States and Western Europe.

The revival of conservatism as a theory of public policy is due more to the lack of competing, consensual theories than to its intellectual rigor or empirical validity. Like other ideological movements, it has reconstructed its own idealization of history mixing myths liberally with facts. One of the most cherished myths about the nineteenth century in the United States is that there was no government intervention in economic and social affairs and, consequently, remarkable technological progress and economic growth, enhancing, in turn, social progress. Historical evidence contradicts this myth. Government intervention was extensive in local and state levels, where most economic activities and social initiatives took place.

The economic–political history of the American telephone industry from its inception until 1934 is examined in this interesting book. More than yet another well-documented case study of government intervention, Stone advances the thesis that public service liberalism was responsible for the many impressive achievements in the nation-building stages and that its revival as a theory of public policy, rather than the indiscriminate institutionalization of conservatism, could assist in resolving certain current problems, such as the savings-and-loan institutions and the cable television industry.

According to Stone, the state role in a capitalist society is not simply intervention versus nonintervention, regulation versus laissez-faire. Policy choices are wider and more complex. They involve not only economic criteria (efficiency being the acid test) but social and political values, as well. If the free market cannot satisfy the desired values, government intervention can help to do so. Intervention can be short, long, or permanent (e.g., the regulation of public utilities) depending on the values to be achieved. In fact, even heavily regulated sectors, such as the telephone industry until 1934, can operate efficiently when they are monopolies. At the core of public service liberalism is a "presumption in favor of free competition that can be rebutted by showing that government intervention is necessary to attain social values that the unfettered market may not achieve" (p. 4). For example, primary and secondary education are now considered as necessary for society's well-being as water distribution, gas for cooking, or electricity. As society modernizes, increasing numbers of services become central to the nation's well-being, presenting opportunities for government–business cooperation. Such cooperation often enables economic efficiency, as well as social progress (as illustrated by the telecommunications industry).

The idea of public service liberalism differs from other forms of government regulation and intervention. When an industry is classified under the heading *public service*, explicit obligations are imposed on it: to serve all who apply and are willing to abide by the reasonable regulations of the firm, to serve adequately, to act impartially to all, and to charge only reasonable rates. These obligations are intended to attain social values in addition to economic efficiency. Conflicts over the practical operationalization of these obligations are inevitable, and the rules covering their application have become more complex and intricate as the volume of cases brought to courts and commissions has increased over time. Furthermore, the research controversy over the extent to which public service decisions tend to favor the regulated industry (the "capture theory") is far from being resolved. To make his theory more tenable, Stone introduces another controversial assumption: "Public decision makers can cooperate in achieving the public welfare not by balancing the demands of interest groups, but by making explicit, reasoned decisions designed to attain predetermined goals" (p. 50).

Public Service Liberalism is a fascinating case study of the history of government–business relations in the telephone industry. The concept is intuitively appealing and more humane than the value of economic efficiency as the exclusive consideration in public policymaking. At present, however, public service liberalism is neither an explanatory, nor a predictive, theory of public policy. More conceptual and empirical research is needed to clarify basic assumptions, contextualize propositions to contemporary structural issues, and explicate testable hypotheses amenable to comparison within and between vital industries.

University of Wisconsin, Milwaukee DAVID NACHMIAS

Mobilizing Interest Groups in America: Patrons, Professions, and Social Movements. By Jack L. Walker. Ann Arbor: University of Michigan Press, 1991. 272p. $39.50 cloth, $14.95 paper.

In January 1990 the profession lost one of its most thoughtful students of American politics, Jack L. Walker, in an automobile accident. At the time he was at the midpoint of a year at the Center for Advanced Studies in the Behavioral Sciences working on a book that was to have been the culmination of a decade-long investigation of interest groups in Washington. An outline of the entire manuscript and several chapters had been drafted. Other significant pieces existed in the form of a distinguished set of papers that had appeared at regular intervals throughout the preceding decade.

A group of present and former students and colleagues (Joel Aberbach, Frank Baumgartner, Thomas Gais, David King, Mark Peterson, and Kim Scheppele) have assembled the pieces and crafted this volume as Walker's legacy to all of us who care about American politics as he did. In the process, they created a book; they have not merely stitched together a set of preexisting papers. Their efforts constitute a tribute to a cherished colleague and friend, a testimony to his ability to inspire students and work with colleagues, and an immense service to the study of American politics.

Based on large-scale mail surveys—conducted in 1980 and 1985—of membership associations active in Washington, *Mobilizing Interest Groups in America* weds systematic data to clear-headed thinking about American politics to probe basic questions about interests groups: Where do they come from? What inducements do they offer supporters in order to stay in business? What influences the choices they make with respect to political strategy? Especially significant are the findings on group mobilization and maintenance. Walker points out the irony in political scientists' adopting the perspective contained in Mancur Olson's path-breaking work and pointing out the barriers to organization just when Washington was witnessing a massive mobilization of new groups into politics. Without losing sight of Olson's fundamental insight that those who wish to start an organization or keep one going must provide some kinds of incentives to supporters, Walker uses multivariate analysis to probe the multiple sources of recent group mobilization. Of particular note is the extent to which contributions from patrons (wealthy individuals, foundations, and the government itself) have facilitated the organization of new groups, especially among those not based in occupational or commercial communities. In addition, in a chapter coauthored by King, Walker presents extensive data about the benefits that group leaders deem important for attracting members to their organizations. The findings not only present a challenge to Olson's by-product theory by demonstrating that according to association leaders, collective goods are important inducements to members but also elaborate the circumstances under which an organization's members are likely to value such nonselective benefits.

Walker also addresses the question of the multiple factors that influence an organization's choice of political strategies. The data in a chapter coauthored by Gais indicate that among other things, groups that operate in what is perceived to be a conflictual policy environment are more likely to be politically active and, in particular, to adopt such insider tactics as seeking influence through established government channels. A chapter coauthored by Scheppele on the litigation strategies pursued by membership associations is particularly subtle. Rather than treating litigation in an undifferentiated manner, Walker presents alternative models to demonstrate the differences among the factors that predict three different forms of legal action: filing amicus briefs, filing suits on behalf of members, and filing class action suits. In addition, he presents an unusually complex picture of the meaning of group litigation for the political process, demonstrating that consistent with the stereotype, the disadvantaged and liberal advocates of public interest causes do go to court to file class action suits but that the groups most likely to engage in litigation are unions and well-heeled organizations in the profit sector whose aims are conservative.

It is difficult to know how to treat the shortcomings in this important book. Presumably, it does not diminish the achievement of those who labored so unselfishly to assert that this is not the same book that Walker would have produced had he lived. Therefore, it is unfair to hold him responsible for failings in a volume that he left unfinished. Still, it would seem churlish to blame any imperfections on six people who worked quickly under difficult conditions to complete a project they did not originally conceive. But clearly, the unfortunate circumstances under which this book was finally assembled exacted a price. With six surrogate authors standing in for a single intellect, the chapters do not achieve the coherence one might have otherwise expected had Walker been able to complete it himself. Also, although the bibliography appended to the book is extensive, several of the chapters are stingy in citing recently published work that reaches similar conclusions.

Some of the difficulties are more intrinisic to Walker's style and to the project itself. Walker tends, as do so many of us who do quantitative analysis of survey data, to reify the findings. Often the strong assertions in the accompanying prose do not contain sufficient reminders to the reader that what is, in fact, being characterized is a probabilistic relationship of statistical significance based upon indicators that are approximations of the concepts for which they stand. Moreover, although Walker's data are superb for answering certain questions, they are less suited for others. Because he surveyed membership organizations, Walker has no data about the thousands of organizations active in Washington (e.g., corporations, universities, and think tanks) that have no members in the ordinary sense. In addition, the poor return rate from labor unions forced him to omit them from most of the analysis. This means that Walker's conclusions about the aggregate set of organizations attempting to influence Washington politics are much less convincing than his analyses of the internal workings of membership organizations. Furthermore, although Walker makes a seminal contribution to our understanding of group mobilization, he neglects a parallel process. The expansion of the Washington pressure system over the past few decades has resulted not only from the birth of new groups but also from the politicization of existing, but previously apolitical organizations. Their movement into politics has had a substantial impact on the character of organizational activity in Washington.

I wish I could have made these points to Jack himself while the book was still in manuscript and congratulated him personally on his achievement. Like his six comrades who undertook to complete what had already come so far, I shall miss him.

Boston College Kay Lehman Schlozman

Senate Elections and Campaign Intensity. By Mark Westlye. Baltimore: Johns Hopkins University Press, 1991. 265p. $38.50.

The central thesis of this well-written and insightful volume is that elections to the U.S. Senate are a good deal more varied than current wisdom suggests. Against the backdrop of an avalanche of research on elections to the House of Representatives, there has grown a small body of work that describes Senate elections as quite distinct from House contests. The literature presents a view of Senate elections as competitive races with incumbents facing tough policy-oriented challengers compared to House contests in which nearly invisible challengers are ritualistically trounced by popular and well-financed incumbents. Westlye argues that only some Senate contests fit this description.

Senate Elections and Campaign Intensity examines Senate elections over the 1968–84 period to argue that campaign intensity is a fundamentally important variable for differentiating among Senate elections. Westlye groups campaigns as *hard fought* or *low-key* in terms of their campaign intensity, and this distinction becomes the principle organizing concept of his analysis. Campaign intensity is conceptualized as the amount of information available to voters during the election period. There is a potentially troublesome distance between this theoretical definition and the operational measure of campaign intensity, which is based on Congressional Quarterly preelection summaries, augmented (in cases) by actual election returns. Westlye does, however, demonstrate that campaign intensity, as measured, is related to amounts of newspaper coverage in a sample of races.

Hard-fought contests, by Westlye's count, amount to 57% of Senate elections in the 1968–84 period. These generally have more experienced challengers and are the more issue-oriented and ideological battles of the conventional wisdom. The remaining, low-key contests generally have a popular incumbent facing an inexperienced, underfinanced, and ineffective challenger. These elections closely resemble the general view of House elections. The failure of previous research to appreciate the diversity among Senate elections, Westlye argues, stems from researchers' reliance on the National Elections Studies data, which (until the recent special Senate election studies) had too few respondents in the low-key contests. This is because the low-key Senate elections tend to cluster in the less-populated states, which are not well represented in national surveys.

To make his point about the variation in campaign intensity, Westlye arms himself with two important new data sources. One is an impressive collection of state-level surveys from a host of different organizations. Here, he does a fine job of skirting the dangers of data incomparabilities to pull out important comparative generalizations. Second, he draws on content analyses of the full preelection newspaper coverage in a dozen Senate campaigns. His focus on newspapers is well justified in an insightful discussion of the public's sources of news (chapter 3), and his case studies here persuasively demonstrate his point about the considerable variety in campaign intensities across Senate elections. Throughout, there is a skillful blending of his quantitative and case study analyses to reinforce and illuminate his basic argument.

Westlye takes on some important issues of electoral research, and generally does an excellent job of relating these to his organizing *hard fought/low-key* distinction. Ideology and issues, for example, loom as more important factors in hard-fought contests, while partisanship and incumbency are more dominant in low-key elections (chapters 5 and 6). He challenges the idea that state population size is a consistently important variable (chapter 7). Contingency tables are the primary vehicle for presenting his analyses. These make his findings easily accessible to a very wide audience; but in places, regression or probit might have provided stronger evidence for some of his conclusions.

The most sophisticated analysis is saved for the end with the examination of the effects of divisive primaries on general election outcomes. The conclusion is important and persuasive: strong challenges in the primaries increase incumbent vulnerability. The election model Westlye develops for this particular hypothesis is more powerful and complete than anything that precedes it in the book, and it might have been adapted for more powerful tests of propositions offered earlier in the book. It is curious that his final analysis is not tied back to the central distinction.

Westlye seems to assume that mixing elections that vary by campaign intensity is like comparing apples and oranges. However, whether a campaign is hard fought or low-key (or even somewhere in between) is a function of variables we usually want to assess in congressional elections, not an independent factor that can be easily isolated. Thus, while Westlye properly focuses our attention on the important feature of campaign intensity and how it varies in Senate elections, he has not emphasized its endogeneity as completely as he might have.

The main argument of this book is solid and important. Senate campaigns vary a good deal, and research will be better for taking this into account in these and other subpresidential elections. This is a fine book and a useful addition to the burgeoning literature on Senate elections.

Indiana University GERALD WRIGHT

Coming to Public Judgment: Making Democracy Work in a Complex World. By Daniel Yankelovich. Syracuse: Syracuse University Press, 1991. 290p. $34.95 cloth, $16.95 paper.

In *Coming to Public Judgment*, Daniel Yankelovich begins with a premise with which few would argue: the quality of American public opinion could use substantial improvement. But the book is actually far more interesting than this common thesis might suggest. Yankelovich calls for a more engaged, thoughtful public dialogue; but unlike most who decry the quality of American public opinion, he emphasizes the limits of information in remedying this situation.

His argument revolves around several major concepts. Foremost among these is public judgment, which he describes as "the state of highly developed public opinion that exists once people have engaged an issue, considered it from all sides, understood the choices it leads to, and accepted the full consequences of the choices they make" (p. 6). If this seems a tall order for the public as most political scientists know it, Yankelovich has a set of practical suggestions for bringing the public closer to his ideal.

Coaxing public judgment into existence is seen as an urgently needed remedy for a widening gap between the public and policymakers. As the formal institutions of government have become increasingly democratic in terms of the participatory roles that they extend to average citizens, the public has had less actual influence on political decisions. Yankelovich claims that this problem is of such proportion that the distance between America's elites and its average citizens is as great or greater than the distance separating the aristocracies of prerevolutionary France and Russia from average people in their countries. Although this may strike readers as an overstatement, it seems less so when one comes to understand that he is speaking of attitudes, rather than differences in class or power. He refers to a "form of intellectual snobbery" in which political elites (and academics) denigrate public opinion and fail to respect its capacity for wise decision making (p. 4).

The reason for the public's lack of influence is a phenomenon he dubs *creeping expertism*, that is, the increasing tendency to allow expert opinions to guide policy decisions. This practice is deemed dangerous because expert opinion is qualitatively different from public judgment. Public judgment goes beyond well-informed opinion to incorporate values and ethical aspects of policy decisions, rather than emphasizing their factual, informational components. Creeping expertism is seen as a symptom of the *culture of technical control*, a societal emphasis on specialized knowledge, science, and empiricism that has undermined other modes of knowledge. Although Yankelovich's ideas in this vein are similar to other critiques of empiricism, they are particularly interesting as argued by a person whose livelihood is steeped in numbers.

Yankelovich's most intriguing and controversial claims occur in his discussion of knowledge, opinion, and public judgment as a form of knowledge. Drawing on philosophical and historical writings, he concludes that opinion is what society relies upon in the absence of knowledge; it is an inferior substitute that we make do with in a pinch. The underlying assumption is that if people were given more information, their opinions would come closer to knowledge and thus would be better opinions. But more information is not the answer, in his view, because public judgment is more than simply well-informed public opinion. He advocates moving away from a quality-as-information model for judging opinion and toward a model that values consistency, stability, and the extent to which people have thought through the consequences of their positions.

The book covers the expert side of the power imbalance between experts and the public less thoroughly. "Expert opinion" is depicted as if it were all of one cloth. Yankelovich implies that the objective character of technical expertise should lead all experts to roughly the same conclusions. Differences between mass and expert opinion are noted as if it were possible to identify a singular expert opinion on any given issue and evaluate its distance from the public consensus. This is troubling because it downplays the extent to which experts have become pawns in political battle. As any television news program demonstrates, experts can be found to espouse almost any policy position. The author contends that the imbalance between public and experts results from a cultural emphasis on scientific information, not from a power struggle; but the increasing importance and visibility of experts seems inextricably tied to struggles for political power.

Toward his goal of improving public judgment, Yankelovich provides Ten Rules for Resolution—instructions, essentially for leaders, on how to encourage the development of public judgment. These are useful suggestions, and the goal is clearly a worthy one; but the underlying assumption is that leaders are disinterested, which most leaders clearly are not, nor should they be. Thus, the goal of creating a national consensus becomes one of persuading people to a particular conviction. Although Yankelovich points out that cultivating public judgment has some benefits for leaders, it remains unclear what would motivate them toward this end when they would seem to have far more to gain from manipulating public opinion.

Despite antiempirical undercurrents, Yankelovich argues that polling can be utilized in the service of democracy when it is used to elicit public judgment, rather than mass opinion. Of particular interest to those concerned with theories of the survey response is his contention that stable opinions can and do exist and that they can be tapped through traditional survey methods. This is an interesting contrast to survey response theories suggesting that the search for pure underlying opinions is a misguided one.

In *Coming to Public Judgment*, Yankelovich provides an insightful commentary on the nature of contemporary public opinion, as well as a normative prescription for improving it. The book should be of great interest to political leaders, students of public opinion, and anyone concerned with the relationship between public opinion and policymakers.

University of Wisconsin, Madison DIANA C. MUTZ

COMPARATIVE POLITICS

Currents of Radicalism: Popular Radicalism, Organized Labour, and Party Politics in Britain, 1850–1914. Edited by Eugenio F. Biagini and Alastair J. Reid. New York: Cambridge University Press, 1991. 305p. $49.50.

The title of this collection of essays does not indicate that in its pages a battle is being waged—a battle over the past and, by implication, over the future of the British labor movement. For both editors and contributors, the history of the British labor movement in the nineteenth century has been needlessly and illegitimately divided into three distinct periods: a period of early radicalism (Chartism); a period of stagnation in which Labor was economistic in its demands and politically beholden to the Liberal party (old unionism and lib.-labism); and a period of the emergence of a genuine, independent labor politics (new unionism and state socialism).

The culprit responsible for this division is what the editors in their introduction call the "Marxist assumption" in the literature on the British labor movement, an assumption tacitly shared even by non-Marxist historians. This "assumption" (*paradigm* would have better captured the editors' intended meaning) tends to evaluate the experience and history of the British labor movement in the nineteenth century according to a priori notions of class struggle and state socialism as yardsticks of "maturity" ("identity" here would have been the better term): the closer the labor movement appears to conform to these yardsticks in its politics and programmatic goals, the more "mature" (genuine) it is deemed to be (p. 3). Against this orthodoxy, the 11 essays in this volume, ranging from political–biographical reassessments to studies of tax and labor law reforms, attempt to cast a wider net over the history of the British labor movement, integrating it in a larger history of British radicalism. Thus, nonconformity, liberalism, and trade unionism are viewed as three continuous "currents" in the history of the British labor movement that present a link to an original British radical tradition, as well as a source of continuous strength and vitality. To the editors and contributors of this volume, the future of the British Labour Party lies squarely in the renunciation of "socialism" and the general acceptance of the "currents of radicalism" presented in the volume.

Despite the high level of historical scholarship displayed, I have my doubts about the link between the essays and the general argument developed by Biagini and Reid in their introduction. To begin with, as the editors themselves realize, the contemporary focus on working-class *experience* (Who were those workers? What did they really do, think, read, etc.?) is significantly indebted to the (neo-)Marxist historians' dissatisfaction with the concepts of class and class struggle as *empirically* meaningful entities (p. 14). Furthermore, the emphasis on a distinctively British radical tradition that was fueled by "notions of rights, justice, 'fairness', independence, dignity and individual freedom" (Thane, p. 269) and that was hostile "to the intrusion of the state" (McWilliam, p. 60) is not that new or revolutionary.

Even more importantly, the essays themselves provide plenty of material with which to contest the positive spin editors and contributors want to give to the thoughts, ideals, and practices that the "Marxist assumption" casts aside as "immature" or "labourist." Thus, Rohan McWilliam, in his fascinating essay on the Tichborne case, sees the irony of a popular mass movement that was concerned with restoring the inheritance to an aristocrat—even if that mass support was based on the popular notion of "fair play" and an "alternative morality" (p. 56). Jon Lawrence, in his equally fascinating case study of Wolverhampton, explains working-class support for Conservatism by "a powerful suspicion of party organization and the party 'wire-puller'" that was channeled into support for "a new type of populist Tory politician" who could claim to be independent from the ruling Liberal apparatus. Yet Lawrence has to admit that this was not a particularly smart move on the part of Wolverhampton's workers, who were basically dazzled by antiestablishment rhetoric (p. 75, 84).

Ultimately, of course, the age-old question of reform or revolution (Whither Labor?) cannot, and will not, be settled in a collection of essays. Nevertheless, political scientists interested in the history, ideology, and practice of the labor movement might miss in this volume sophisticated nods to industrial capitalism and system-transcending reforms. In my view, it is certainly not enough to laud Robert Knight's "instinctive libertarianism," as A. J. Reid does, by a simple reference to the contemporary British "practical experience" with "centralised welfare and nationalised industry" (p. 238).

The essays in this collection present a vital contribution to a general revision process under way in the fields of labor movement history and labor movement politics. Bolstered by a useful and sharp introduction, this book should interest both historians and political scientists working in those fields.

Rutgers University F. Peter Wagner

Of Time and Power: Leadership Duration in the Modern World. By Henry S. Bienen and Nicolas van de Walle. Stanford: Stanford University Press, 1991. 215p. $25.00.

The question of political survival—why some political leaders hold power longer than others—has been at the heart of political inquiry at least since Machiavelli penned *The Prince*. Although political survival has been the subject of a considerable amount of empirical investigation, this small but fascinating book is the broadest, most rigorous empirical consideration to date of the varying political life spans of national leaders.

The book is divided into two parts. The first part is a careful consideration of the possible effects of a number of factors on the political life span of individual national political leaders. The second part of the book presents the entire data set of 2,256 national leaders from the 167 countries upon which the study is based.

Using the statistical techniques of survival analysis as developed in demography, Bienen and van de Walle draw upon two broad sets of variables to estimate political life spans. The first set of variables taps attributes of the leaders and the situations in which they found themselves. Here they recorded, for example, measures of age, temporal position in the leadership queue, whether the leader came to power by constitutional or nonconstitutional means, and whether they had a military or civilian background before coming to power. The second set is directed toward measuring characteristics of the leader's country and include, among other things, prior colonial rule, economic growth, income levels, the magnitude of ethnic conflict, and size of population.

The fundamental finding emerging from this study is simple, important, and intriguing. The best predictor of continued political survival is prior survival. Just as the cold, impersonal statistics of actuarial tables say, those who have survived have the best chance of surviving longer. There are some small wrinkles to this finding across different regions, but the result is generally robust.

Why is this the case? At least two possibilities present themselves. One is that those who manage to survive some minimal undetermined length of time successfully consolidate their power and use it to increase their tenure by suppressing the opposition and/or rewarding their supporters. The rival to this is a sort of Darwinian explanation in which "less clever leaders get winnowed out earlier" (p. 98). Although it is disappointing that Bienen and van de Walle do not pursue these two models of survival further, it is nonetheless encouraging that they are able to establish the basic accuracy of their empirical generalization across a broad spectrum of time and space.

The second part of the book reports in considerable detail the main elements of the data analyzed in the first part of the book. The data they report (understandably more limited than the total data set collected for the entire study) identify the state, its geographic region, the leader, the length of time the leader held office, the year of entry, whether entry/exit was constitutional, whether the leader was a member of the military, and how many separate times the leader was in power.

Although the data are extensive and carefully reported, they are not without their curiosities and problems. It is, for example, curious that although data for many Third World nations (particularly in Latin America) extend well back into the nineteenth century, the data for the developed nations typically begin in the early part of this century. Thus, O'Higgins' tenure in Chile is recorded beginning in 1817; but British prime ministers begin only with Balfour in 1905. No explanation is offered for this, nor is there any speculation on what effect such a gap might have on the findings.

Doubtless, anyone who is familiar with history will find things with which to quibble in the data. For example, is it accurate to regard the reign of Haile Selassie (1930–74) to have been uninterrupted, considering that he was forced into exile in London by the Italian occupation (1936–41)? Or can we consider Stalin as actually being the leader of the Soviet Union as early as 1924? Or how can it be recorded that de Rosas left power constitutionally in Argentina in 1852 when he fled after losing the battle of Caseros to insurgents abetted by both Uruguay and Brazil? There are a few other points on which the data seem questionable, but a data set of this sweep is inevitably open to doubt on the part of those with different information or views.

The limits of the analysis and a few problems with the data should not, however, obscure the fundamental contribution of this book to our understanding of political survival. Moreover, it points the way for further exploration of the data and the testing of a variety of models that may account for the differing fortunes of political leaders.

University of California, Davis RANDOLPH M. SIVERSON

Crossing the Industrial Divide: State, Society, and the Politics of Economic Transformation in Malaysia. By Alasdair Bowie. New York: Columbia University Press, 1991. 222p. $40.00.

Bowie has written an intellectually stimulating case study with interesting conclusions that are relevant to a larger sample of countries. In this book, he sets for himself the task of explaining how and why development strategies change in late-industrializing, ethnically divided, Third World nations. In Malaysia (the case he considers), development strategy has changed from a market-led approach during the country's first decade after independence (1957–69), to a mixed market–regulatory policy during the 1970s, and finally to a state-centered strategy for most of the 1980s.

In seeking an explanation for these changes, Bowie first reviews the literature on industrial policy choice in Europe and finds that extant explanations for policy change have little to offer for an understanding of the Malaysian case. Explanations that look to the material interests of actors (whether individually or aggregated in the form of the usual intermediate associations) cannot explain policy change in cases where the principal actors have in common their communal, not material, interests. These communal interests combine diverse material interests and cross class lines. Structural explanations emphasizing either domestic or international factors are inadequate, since the major policy changes occurred in 1970 and 1980 without any corresponding or precipitating structural changes. Likewise, a political culture framework is found to be inappropriate for explaining change that occurred within one generation in a single country.

With the literature derived from the European experience unable to explain his case, Bowie returns to, and reformulates, a more societal-based explanatory framework. In this effort, he joins other recent graduates of the University of California, Berkeley in providing a needed corrective to the more state-centered approaches to explaining Third World developments. This seems especially appropriate for the study of the rapidly industrializing parts of Southeast Asia, where the state has generally not been as central to industrialization as elsewhere in Asia. Bowie's position is that "changes in Malaysian economic development strategy can best be explained as products of the changing nature of a communal settlement that has prevailed in Malaysia since 1957" (p. 9).

The original settlement, engineered by the British at the time of independence, gave the Malays control of the political system. In exchange, the Malays agreed to accept the ethnic minorities as legitimate residents of Malaysia and not to use their control of the state to challenge minority economic interests. During this market-led period, the Chinese and foreign-owned corporations flourished.

A market-led development strategy did not, however, advance Malay interests, especially those of the poor. This contributed to the 13 May 1969 communal violence, which, in turn, contributed to the revision of the communal agreement and the emergence of the New Economic Policy of the 1970s. In effect, the result was a mixed market, regulatory, affirmative action program for the Malays.

The next shift in policy in 1980 led to a far more activist, state-led process of heavy industrialization. The economic justifications for this policy shift are acknowledged, but Bowie argues for the importance of political considerations. In essence, the Mahathir government, given its ethnic base, was sufficiently independent of Chinese economic interests to use state power to promote a Malay-controlled set of heavy industries.

This last portion of Bowie's argument is less well substantiated; the evidence of Malay pressure for a more activist government role is less clear than in the earlier, 1969 period of communal violence. Since his explanatory framework looks for causal variables in society, it would have been helpful to see more evidence of how the Malay community influenced their communal leadership. Lacking this kind of supportive evidence, the reader is left to speculate about governmental claims to be advancing communal interests. This becomes more important in the light of scholarship such as that of Jomo Kwame Sundaram, which asserts that a Malay administrative elite has, since the end of British colonialism, used its position in government to transform itself from a "statist petty bourgeoisie into a statist capitalist class accumulating on the basis of control of state power" (*A Question of Class* [1988], 289).

Thus, for the Malaysian specialist, Bowie's book will leave unanswered the question of who are the primary beneficiaries of policy choice. However, Bowie's goal was not to write a political economy of policy outcomes. He wanted to explain policy choice, and his major contribution is to the scholar interested in the comparative political economy of development in the Third World. For this audience, Bowie has written a persuasive—even compelling—argument that we must be more sensitive to societal explanations for policy choice, especially in ethnically divided nations.

Ohio University GARY HAWES

Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America. By Ruth Berins Collier and David Collier. Princeton: Princeton University Press, 1991. 877p. $75.00 cloth, $19.95 paper.

This is a monumental work, a tour de force. It is one of the most important books in the field of Latin American politics in several years. The "Collier thesis" will now have to be confronted by all students of the role of state–labor relations in political development.

The Colliers emphasize the type of labor incorporation as a key to the modern political trajectory of developing nations. That focus is convincing, whether explaining the phenomenon of Juan Perón in Argentina or Salvador Allende in Chile. To make their case, the authors use the comparative historical method to provide excellent coverage of eight countries. Their achievement will be welcomed by historians and sociologists, as well as political scientists. It will make valuable reading for scholars concerned with democracy, dictatorship, corporatism, the state, political parties, or trade unions.

Their central argument is that countries differ significantly as to whether the working class was incorporated by the state or by political parties. Whether labor was admitted to the political arena under government control or through mass mobilization set in motion a distinctive sequence of conflict and accommodation. The initial period of institutionalization and legalization constituted a "critical juncture." It followed a spurt of reform and tended to crystallize certain types of political parties and systems. That period of assimilation was succeeded by predictable political consequences in the short-term "aftermath" and long-term "heritage." The Colliers trace these processes both from the top down and from the bottom up. They do so in the eight Latin American countries with the strongest development of an urban working class. They place those cases in the context of very wide reading on comparative politics and labor movements in other parts of the world.

Their classification of cases is surprising, provocative, and fruitful. The Colliers cleverly juxtapose similar patterns in different countries in different time periods. (For example, for labor incorporation under authoritarian rule, the 1920s in Chile resembled the 1930s in Brazil.) Although bound to be controversial among country specialists, their typology will change how scholars think about the timing, sequence, and direction of alternative paths to development in Latin America. The Colliers establish four categories: (1) state incorporation (Chile and Brazil); (2) electoral mobilization by a traditional party (Uruguay and Colombia); (3) labor populism (Peru and Argentina); and (4) radical populism (Venezuela and Mexico). These four contrasting modes of bringing labor into legitimate politics produced, respectively, (1) multiparty polarizing systems, (2) electoral stability and social conflict, (3) a stalemated party system, and (4) an integrative party system.

These pairings illuminate unusual parallels between political systems. Although Chile possessed much stronger parties and democratic traditions than did Brazil, both countries underwent political polarization and fragmentation that ushered in durable antilabor authoritarian regimes. Uruguay succumbed to military dictatorship longer than did Colombia and suffered less private violence, but both maintained electoral domination by multiclass parties with weak links to labor organizations. Although highly different economically and ethnically, both Argentina and Peru saw their attempts at working-class incorporation result in the banning of the major labor parties and the repeated breakdown of democratic governments. Meanwhile in oil-rich Mexico and Venezuela, although the first was a one-party and the second a two-party system, unions in both cases were tied to centrist parties and became stabilizing influences. By spotlighting these similarities between otherwise contrasting political histories, the Colliers underscore the autonomy of some political factors and the decisiveness of the form of integration of trade unions.

The only drawbacks to this book are the excessive length and repetition, as well as the imprecision of some of the organizing concepts. Particularly fuzzy are the different historical stages and, above all, the notion of *critical juncture.* It is difficult for an objective observer to determine when such an inflection occurs, what causes it, how long it lasts, and what it produces. Although the authors make a convincing case that such watersheds, or turning points, profoundly affect political development, they are unable to sharpen its definition much beyond "a period of significant change" (p. 782). Their identifications of critical junctures in each country case are persuasive, but the concept remains slippery and elusive as a traveler across borders in comparative politics.

Quibbles aside, this extraordinary synthesis will stand as a starting point for Latin Americanists for many years. For students of other world areas, it should be a major source of reference and debate. As a bonus, the authors provide an appendix listing all the heads of state since 1900 and a stupendous bibliography of secondary sources. This book is further enhanced by the clarity of the writing and arguments. It will undoubtedly find its way into many libraries and classrooms.

University of California, San Diego PAUL W. DRAKE

The Accidental Proletariat: Workers, Politics, and Crisis in Gorbachev's Russia. By Walter D. Connor. Princeton: Princeton University Press, 1991. 374p. $39.50.

In the political section of a March 1992 issue of the *Independent Newspaper*, a Russian biweekly, the respected labor sociologist, Leonid Gordon, asks a question whose answer is likely to be of central importance in determining the outcome of political reforms now underway in that country: "With tough reform days ahead, whose side will the workers take?" Walter D. Connor's excellent study of the emergence of a Soviet working class and its implications for politics in the former USSR does not try to offer a definitive answer to this question, but it does provide us with the best basis to date for informed speculation.

A key to Connor's thesis can be found in the title of his book. He contends that industrial development under Stalin unintentionally, but inexorably, created a social basis for the emergence of a genuine working-class movement in the USSR. In the post-Stalin period, the hereditization and greater education of workers made possible by industrialization laid the seeds of a class consciousness that began to germinate under Khrushchev and Brezhnev but came to fruition only in the Gorbachev era. According to Connor, the Soviet proletariat had gradually come to realize that their interests were subordinated to those of a new set of bosses, who claimed legitimacy by ostensibly ruling in their name. As they began to acquire consciousness of the division between "us" and "them," opposition along class lines became unavoidable, although it was rarely expressed openly until Gorbachev's leadership. That the Soviet proletariat acquired *class* characteristics, despite every effort by the regime to ensure the opposite, leads Connor to label them "accidental." That they are finding their voices at a time of great economic trouble could seriously complicate the introduction of market-oriented reforms, many of which can easily be portrayed as "antilabor." This is what gives particular significance—even urgency—to Gordon's question and to Connor's book.

The theoretical framework for this work is minimal and employs the four criteria for class consciousness set forth in Michael Mann's *Consciousness and Action among the Western Working Class* (1973). After a historical overview of the Stalin years, Connor examines Soviet labor policies and practices from 1953 to 1988 for evidence that these criteria are becoming apparent. Most of the book is given over to chapters detailing worker career patterns, wage equalization, employment, and control of the workplace. In each case, the reasons for growing worker dissatisfaction are shown; and Connor makes a convincing case that class identity and class opposition were well advanced even before *perestroika* and are only likely to be accelerated by the economic reforms currently under way. Particularly valuable is the extensive documentation of strike activity before 1985, which the author has compiled. The final chapter and an epilogue chronicle events through the spring of 1991.

To say that the theoretical framework is minimal is not to say that the book is ill-informed theoretically. The author is clearly aware of the controversy over state-versus-society-based explanations for political change in the former Soviet Union. Although the weight of the analysis is intended to show that long-term societal transformations are driving contemporary political change, Connor is not unmindful of the particular role played by the state in the Soviet context (p. 6). The state's ability to manipulate the terms of the "social contract" or, when necessary, to employ coercion account for the general absence of a politically active working class prior to 1985.

The information on which this book is based comes not from extensive field research (a brief visit to Moscow in early 1988 is noted) but from a magisterial review of Russian- and English-language source materials. As a result, the picture we get of the Soviet blue-collar worker lacks the feel of the shop floor, although Connor makes good use of anecdotal material from published sources. In its place is a surfeit of descriptive and statistical information—meticulously noted, but often leavened only by the author's felicitous writing style. This is not a book for the casual reader.

I find Connor's central thesis persuasive and of critical importance for anyone who wants to understand Russian politics today. One may differ, however, with Connor's emphasis on what might be called blue-collar conservatism. He pays considerable attention to the right-wing United Worker's Front and to the attempt of the older trade unions to create a new constituency among the workers by appealing to antimarket populist sentiments. He also finds relations between workers and intellectual reformers characterized chiefly by mutual antipathy (pp. 227–29). Much less space is accorded to proreform worker movements. If, as Connor frequently points out, most of the voting-age population are workers and if there is, indeed, hostility among workers toward reformers who favor market solutions, why did the UFT, and conservatives generally, not do better in the elections of 1990? And why did Yeltsin win 54% of the presidential vote in 1991?

The article from which Gordon's question is quoted portrays a more differentiated proletariat than does Connor's book—a proletariat in which support for democratic market reform is at least as strong as opposition to it. This, of course, does not contradict Connor's main thesis of an emergent and politically relevant working class. But if Gordon is right, our assessment of how that working class will affect Russian politics would be more hopeful.

Villanova University JEFFREY W. HAHN

The Invisible State: The Formation of the Australian State, 1788–1901. By Alastair Davidson. New York: Cambridge University Press, 1991. 329p. $65.00.

This account of Australian state building is likely to be controversial. Alastair Davidson uses conceptual tools forged in the thought of Antonio Gramsci and Michel Foucault, among others, to move beyond a more traditional descriptive approach to political history. He focuses on the production of citizens in Australia and how this particular citizenry shaped the political institutions that ultimately emerged. In a striking conclusion, Davidson argues that the result was and is an Australian state that lacks popular sovereignty and consequently is not a democracy.

His chronological presentation begins with the conditions in England leading to the use of Australia as a penal colony. Two groups not usually considered central to state building are highlighted: the criminal class that grew up around the perimeter of London (to which most of the transportees belonged) and the theorists and administrators of punishment, who were responding to the growing problem of crime. It was from the descendants of these two groups—the convicts and their jailers—that the raw material for Australian citizens would be drawn. Consequently, the particular cultural and political values of these groups became the British legacy shaping Australian political development. The values that went across the Indian and Pacific oceans to Australia were not the same as the fear of strong government and commitment to personal rights that came across the Atlantic to the United States. This is at the root of why Americans revolted and insisted on popular control of government, while Australians, ultimately, did not.

Given these beginnings, it is not surprising that the process of colonization and expansion was unique. In a community where control had traditionally been the first concern, the freedom implied by a frontier posed problematic issues of personal responsibility, property, and even family structure. Davidson describes how these issues became administrative concerns, resulting in the authoritative construction of everything from work habits to marital relations. Against this background, various segments of Australian society vied for power as political institutions evolved during the nineteenth century. The result was that the inherently hegemonic place of the law in a prison was transformed into a traditionally hegemonic place for the people involved in the judicial system in the postprison community. Lawyers emerged as the dominant group in Australian public life; and in a series of conflicts leading up to the Constitution of 1901, the relative place of political and legal authority was established, effectively carving out a space for political activity while maintaining the dominance of the law. Even in the central political document establishing the state, the long arm of the law was institutionalized; for leaving critical issues of authority vague left ultimate sovereignty to the courts.

Davidson has made an important contribution to the state-building literature by using a general theoretical perspective to explore a unique historical case. Indeed, it inspires the kind of inconsistent criticism that only surfaces when a book is really worth reading. On one hand, the sheer volume of detail can be overwhelming, so that the multiple strands of the description do not always link up easily to the major thread of the argument. At the same time, it is impossible not to wish for more, especially more comparisons with other countries and more contrasts with related literature. The obvious comparison is with the United States, which also had a frontier and ended up with judicial review. Bringing wider experiences and theoretical concerns into the discussion could be valuable in many ways, however, for Davidson's work falls within the purview of some very significant efforts by other scholars. For example, consider the interesting questions that would be provoked by trying to situate the Australian experience into the framework used by Barrington Moore in *Social Origins of Dictatorship and Democracy* (1966) or by examining the early authority structure and relation of citizens to political organization in Australia and looking for parallel insights in the work done by Perry Anderson in *Lineages of the Absolutist State* (1975) and Reinhard Bendix in *Nation-building and Citizenship* (1964). Comparison with a nonrevolutionary change in state structure during the same time period like that described by Stephen Skowroneck in *Building A New American State* (1982) could address issues of continuity in the administrative side of state development.

These are, of course, unfair expectations; but it is important that this work not be classified as simply a history of Australia. The wider significance of Davidson's achievement is the way in which he supplies a new dimension to the discussion of fundamental issues such as political development, state formation, sovereignty, and democracy. These are complex and difficult issues; but when history and political science can be brought together to mine generally significant insights from unique cases like the development of the state in Australia, real progress can occur.

Pennsylvania State University JAMES L. CURTIS

The Japanese Voter. By Scott C. Flanagan, Shinsaku Kohei, Ichiro Miyake, Bradley M. Richardson, and Joji Watanuki. New Haven: Yale University Press, 1991. 497p. $45.00.

This book sets out to bring the study of Japan into the mainstream of cross-national and comparative research on voting behavior. Rather than treating Japan as the unique case of non-Western industrial democracy, the authors apply theories derived from Western Europe and North America to the Japanese case. It accomplishes, however, far more than being merely "Voting Behavior: The Japanese Case." In addition to applying European-style sociological models and American-style psychological models, the authors develop a *social network model* that owes its heritage primarily to Japanese studies. Though derived from Japanese studies, the social network model is also a comparative model, supported by evidence from Europe and North America and meant to be applied to voting behavior in any industrial democracy. Thus, the authors both apply Western models to Japan and also challenge Western researchers with a "Japanese" model.

The volume contains a trove of valuable insights into Japanese voting behavior, many of which also raise questions about Western voting behavior. Perhaps the most surprising findings concern family versus adult socialization. Miyake finds that "when parents' partisanship conflicts with one's group partisanship, the latter tends to prevail" (p. 210). Miyake's exploration of partisanship in Japan is also stimulating and informative on several levels. Less surprising are Kohei, Miyake, and Watanuki's findings on the effect of issues on the vote, concluding, "If over a quarter of the voters have no positions on the issues, over half are unable to identify any party that represents their views, and over three-quarters fail to link their issue preferences with the party they voted for, then it seems unlikely that a study of issues will contribute greatly to an explanation of voting outcomes" (p. 284). Watanuki's treatment of social structure and voting behavior provides an excellent analysis of the development of the postwar cleavage structure. Flanagan's final chapter uses the theories developed in the volume to analyze the most recent electoral results and to think about the future of Japanese politics.

The two most exciting chapters deal directly with the social network model. Although we do not, in the end, arrive at a coherent new model of Japanese voting behavior, we do make significant progress in that direction. The literature on Japanese politics contains many studies of social networks from the candidate's point of view. The picture we get from these studies is of a tightly knit, candidate-centered voting bloc (*jiban*) that effectively insulates the candidate from electoral tides. These two chapters give us the first systematic look at social networks from the voters' point of view and find little support for the picture of a highly organized hierarchical social structure.

Scott Flanagan deals with the mechanisms of social network influence on Japanese voting behavior, distinguishing assimilation effects, reference group effects, opinion leader effects, and mobilization effects. He finds that "Japanese social networks will in many cases supply an apolitical voter not only with name recognition and a favorable image for a particular candidate but also with a strong motivation to vote" (p. 158). Bradley Richardson analyzes influence communications and finds that "the choices of roughly one-quarter to one-third of the Japanese electorate are directly affected in some way by influence communications processes in House of Representative elections" (p. 355). Not only are these proportions smaller than the literature had led us to expect, but candidate images prove more powerful than personal requests. In the 10% of the cases in which there was a conflict between these two forces, "candidate images prevailed over requests three times as often as requests overwhelmed candidate images" (p. 361). Candidates may have effective organizations but they seem to consist of activists and opinion leaders and do not include a large number of loyal voters.

These findings bring our image of the Japanese voter closer to our image of voters elsewhere. Instead of a "feudalistic" personality imbedded in a social structure so tight that it often renders individual preferences irrelevant, the Japanese voter emerges as relatively unconcerned about politics, with political beliefs and behavior influenced by many different factors. The picture that emerges is complex and not particularly complimentary but is completely recognizable as a variant on the theme of voting behavior in industrial democracies.

University of Alabama STEVEN R. REED

Parties and Voters in France. By John Frears. New York: St. Martin's, 1991. 240p. $49.95.

For the first 20 years of its existence, the French Fifth Republic was regarded suspiciously by domestic and foreign observers: How could a political system so deliberately shaped to conform to the political philosophy and skills of one man—Charles de Gaulle—survive the disappearance of that great man and his immediate successors? Well into the second decade of its existence, many still talked of the Fifth Republic as only a "parenthesis" in the history of French democracy. However, in the last 10 years, the regime has successfully met two key challenges: the transfer of power from the Gaullists and their allies to the Left and the division of power (*cohabitation*) of a president and the parliamentary majority from political adversaries.

In his latest book, John Frears points to these events as confirming conclusively the strength and durability of the regime and the breadth of cross-partisan support for the institutions of the Fifth Republic. He suggests that this may very well end the kind of regime instability that has characterized French political history for the past two hundred years. He argues that the consolidation of an effective and simplified party system played a major part in these successes. Frears points to the emergence of parties with the capacity to form and support governments in power as the major innovation of the Fifth Republic and the source of the strength of its institutions. He argues, further, that it was the transformation of the Gaullist party from one of personal supporters of General de Gaulle into a large, catch-all party that brought together almost all the previously contentious strains of the French Right and Center. The early successes of Gaullism created a "majority habit" that compelled opposing parties to unite and seek to develop their own majoritarian potential. Thus, Frears sees the changes in the Right as pressing the parties of the Left to change and to unify.

After presenting these arguments in a persuasive manner, Frears then provides separate chapters treating the background, policy positions, electorates, and organizational structures of the politically significant parties and party coalitions of contemporary France. Frears includes discussions of some parties that are relatively neglected, such as the French Greens. Later chapters describe the rules and procedures for presidential, parliamentary, and local elections and referenda and give brief descriptions of the issues and major contenders in each national election since 1958.

Frears' contentions about the importance of party change in explaining the success of the Fifth Republic and the key role of the Gaullists in bringing about these changes are briefly but persuasively presented. However, the major strength of this book is the valuable resource it offers students and scholars on the overall party system and election scene in France. It is the only up-to-date general survey on this subject in French or English. It is clearly written and accurate. The chapters on the individual parties are rather short; and a longer bibliography would have been helpful in directing those who need more information to more comprehensive studies.

This is not a book that applies sophisticated theories on political parties to France, nor does it report on important new field research. Specialists on French politics will find little new. But the Frears book does do something that we sometimes neglect in American political science by providing a reliable, readable, and thoughtful synthesis of the theoretical and empirical research on a key set of political actors. As such, *Parties and Voters in France* will serve as a valuable resource for students and scholars.

Purdue University FRANK L. WILSON

Revolution and Rebellion in the Early Modern World. By Jack A. Goldstone. Berkeley: University of California Press, 1991. 608p. $34.95.

Living in an age when new-style revolutions have toppled regimes from Bucharest to Budapest, we find ourselves once again (as in the days of Vietnam) seeking to explain why and how revolutions occur. Familiar theories of imperialism (Eric Wolf, James Scott), class conflict (Barrington Moore, Jeffery Paige), relative deprivation (Ted Gurr, James Davies), or state incapacity (Charles Tilly, Theda Skocpol) seem less applicable to the revolutions of 1989 than to the "great revolutions" that occurred earlier in this century. Perhaps our models of revolutionary change will have to become more historicized, more tailored to the specifics of different periods and cultures.

Jack Goldstone, in his stimulating study of revolution and rebellion in early modern Europe and Asia, makes a similar point. He notes that most theories to date have been "concerned with explaining twentieth-century revolutions, in which the collisions of international colonial and economic forces with traditional regimes played a major role. . . . The theory of revolutions has yet to address early modern political crises in their own right" (p. 23). Goldstone further criticizes previous work for neglecting "the impact of cultural differences, without which one can hardly comprehend the varying effects of state breakdown in early modern Europe and Asia" (p. 23).

For his own part, Goldstone offers a "demographic/structural model" of state collapse, arguing that revolutions occurred when population pressures overwhelmed intransigent political, economic and state institutions. This is not crude Malthusianism; Goldstone concentrates on "the *distributional* effects of relative shifts in population and resources, rather than on massive shortages for whole societies" (p. 37; emphasis original). Moreover, he sees population pressure as developing in a pattern of recurrent waves (driven by changes in climate, disease, food supply, etc.), rather than in linear fashion.

Goldstone's focus on early modern uprisings, on ecological crises, and above all on European–Asian comparisons (the English Revolution, the French Revolution, the crisis of the Ottoman Empire, and the transition in Ming–Qing China) brings a fresh perspective to the study of rebellion and revolution. The chapters on England and France are impressively detailed, presenting both qualitative and quantitative evidence to raise questions with prevailing scholarly interpretations of these familiar events. The analysis of the Ottoman and Chinese Empires, though less original, is useful for further debunking the old myth of an East mired in stagnation while progressive revolutions in the West gave rise to democracy and capitalism.

Insightful as his approach is for the study of early modern rebellions, Goldstone seems overly anxious to extend its applicability across time and space. Area specialists will surely shudder at his claim that "the states of early modern Eurasia—including the European monarchies, Russia, China, the Ottoman Empire, and Japan, in roughly the sixteenth through the nineteenth centuries—were not greatly different from each other" (p. 4). And historians are likely to recoil from the temporal leap involved in the assertion that "the study of early modern history suggests that the current U.S. course [of failing to reduce consumption] will lead to heightened factional conflict and economic weakness" (p. 496). Greater attention to the peculiarities of different periods and places would have strengthened, not diminished, the overall impact of the book. As it is, the concluding chapters (one on ideological and cultural factors, the other on lessons for the present) seem like afterthoughts, intended to stretch the conclusions of the study beyond their legitimate boundaries.

Whether or not this volume has much to tell us about the current fiscal predicament of the United States or, for that matter the revolutions of 1989, it is in any case a major contribution to our understanding of earlier uprisings. While other scholars (most notably members of the Annales school of French history) have shared Goldstone's interest in demography and ecology, none has developed the analysis in such rigorous fashion. This is an important book from an innovative and broad-gauged scholar. It belongs on the reading list of anyone interested in the dynamics of revolutionary change.

University of California, Berkeley ELIZABETH J. PERRY

Public Choices and Policy Change: The Political Economy of Reform in Developing Countries. By Merilee S. Grindle and John W. Thomas. Baltimore: Johns Hopkins University Press, 1991. 222p. $38.50 cloth, $13.95 paper.

Ideas and Institutions: Developmentalism in Brazil and Argentina. By Kathryn Sikkink. Ithaca: Cornell University Press, 1991. 263p. $35.00.

Political and economic upheaval have refocused attention on the origins and determinants of major policy shifts; and growing dissatisfaction with reductionist, often agentless explanation has reawakened interest in the roles of individual policymakers and the ideas and values they hold. The timing is therefore auspicious for the publication of this pair of first-rate books, both of which (despite differences in emphasis and intended audiences) address these critical subjects. As Kathryn Sikkink observes, "It is a paradox that scholars, whose entire existence is centered on the production and understanding of ideas, should grant ideas so little significance for explaining political life" (p. 1), while Merilee Grindle and John Thomas note that despite their absence from many explanations of policy reform, "people . . . change policy, and do it from motivations and perspectives that are imbued with personal and professional values that frequently include serious concern for the public interest" (p. xiv).

Both books seek to explain where policymakers' ideas come from and how and how much their ideas and actions shape outcomes. Both offer similar criticisms of existing approaches: societal (class, pluralist, public choice), statist (state autonomy, bureaucratic politics), and rational choice. In general, the authors conclude, such arguments are meaningless without reference to, as Sikkink (p. 243) puts it, the ideas "without which no understanding of interest is possible"; or else they fail to account for the significant room for maneuver that policymakers possess or for how policymakers choose among options within it. Neither Sikkink nor Grindle and Thomas dismiss these approaches entirely or treat ideas and individuals as fully autonomous. Instead, they fashion arguments that locate policymakers, their ideas, and their motivations within historical, institutional, and societal settings, permitting us to assess how specific policymakers come to decisions about specific policies and how and how fully they succeed in implementing their choices, given state institutional capabilities and state–society relations.

Of the two books, Sikkink's *Ideas and Institutions* has the wider academic appeal, fitting neatly into the growing literature devoted to "bringing ideas back in," where it compares well with the work of Ernst Haas, Peter Hall, Aaron Friedberg, and John Odell. Sikkink poses two questions: "Why were similar developmentalist policies adopted by Brazil and Argentina [in the 1950s and early 1960s]? and why did these economic policies have such different outcomes?" (p. xiii). *Ideas and Institutions* offers an excellent study of a critical era in the history of two major Latin American countries during which their development trajectories diverged sharply in ways which still shape their capacities to implement structural adjustment. Sikkink draws on extensive archival research and interviews with many of the key figures. Finally, as befits a book about ideas, *Ideas and Institutions* is gracefully written. Sikkink shows a nice ability to bring high-flown analysis back to earth with a single, telling observation.

Sikkink begins with a précis of her argument and a review of alternative explanations and then moves to a fascinating intellectual history of developmentalism in Latin America as a whole and Argentina and Brazil in particular. The chapters on the developmentalist experiments in each country offer cogent explanations for the very different ways in which developmentalism was adopted, implemented, and consolidated in Argentina and Brazil and challenge pat understandings of this period. In fact, for Latin Americanists, Sikkink's major contribution may be her richly documented addition to our knowledge of this critical, but underresearched, subject. Not content to discuss ideas in isolation, however, Sikkink devotes two chapters to where developmentalist ideas lodged in the Argentine and Brazilian states and to "how differences in state autonomy, state capacity and state purpose influenced the implementation of developmentalist policies" and thus explain "the different economic and political outcomes" (p. 28). A final chapter returns to the role of ideas and the problems of constructing explanations that capture the recursive relationship among objective conditions, material interests, and actors' ideas.

While very good, *Ideas and Institutions* has faults. Perhaps most important, Sikkink does not always demonstrate the need to turn to ideas when more parsimonious explanations exist. She expresses surprise, for example, that Argentine industrialists failed to support the Frondizi government, which was, she asserts, "firmly committed to industrial development," a failure attributed to the distorting "lens" of industrialists' "prevalent ideology" (p. 108). Conversely, she ascribes Brazil's greater success to the support of industrialists who shared the Kubitschek government's developmentalism. Yet Sikkink notes that Frondizi's industrial policy called for a 50% cut in import taxes, which threatened inefficient Argentine firms (p. 109), while Kubitschek's tariff policy called for "strongly protectionist . . . new rates," which protected domestic industry (p. 142).

Though they, too, have academic readers in mind, Grindle and Thomas address themselves first to practitioners. They endeavor to provide a model of the policy reform process in order "to enable policymakers better to manage implementation" (p. 122). They offer not a predictive theory of policy reform but a "framework" that "systematizes thinking" and maps out "the intuitive knowledge of skilled policymakers and public managers" (p. 187). To illustrate their story, Grindle and Thomas use "mini-cases" of policy reform from a variety of Third World countries. They thus achieve that rare hybrid—a readable fusion of abstract argument and nitty-gritty detail that should interest and enlighten students, academics, and policymakers alike.

Public Choices begins with a generic picture of government decision making in the Third World and a critical review of the literature, whose aim is to show that reform is the norm, not the exception, and that policymakers are more autonomous and possess more room for maneuver than is generally conceded. The book then follows the policy process from start to finish, seeking to specify what accounts for variation. Thus, having identified environmental factors and personal characteristics affecting policymakers, Grindle and Thomas analyze agenda setting, focusing on the implications of crisis versus "politics-as-usual." They go on to analyze how these differences shape the decision process in light of the public and bureaucratic characteristics of specific reforms. Finally, they apply their model to the dramatic reforms of the 1980s and "test" it (though this is too strong a word) in 14 mini-case studies.

Public Choices, too, has faults. The dependence on mini-cases is methodologically suspect. They are at best illustrative and cannot provide a test of the authors' propositions. Moreover, their cumulative effect leaves the impression that each situation is unique and that generalization is impossible. This impression is reinforced by the authors' model, which is as complex and ambiguous as reality itself, since it includes, just by way of scene setting, the "ideological predispositions, professional experience and training, memories of similar policy situations, position and power resources, political and institutional commitments/loyalties, and personal attributes and goals" of policymakers and the "societal pressure, historical context, economic conditions, international context, administrative capacity, and other policies" that constitute the context of any given policy choice (p. 184). In other words, while this model may permit the descriptively accurate categorization of all the elements of completed reform initiatives, it possesses no *explanatory* power for an academic analyst and limited utility as a guide to practitioners.

Their problems notwithstanding, these are important, informative, and compelling books. Sikkink must be congratulated for her excellent paired case study and for her contributions to the intellectual history of Latin America and to the effort to understand how and under what conditions ideas shape policy outcomes. Grindle and Thomas offer a critical reminder of the role individual policymakers play not as "creatures of the societies or classes from which they come" (p. xii) but as leaders in the proper sense of the word. Their work should, as they hope, "help generate principles of action for reform leadership" (p. 193) and challenge scholars to look beyond the obstacles to change and engage the reforms that are transforming the Third World.

Rutgers University D. Michael Shafer

Public Enterprise in Kenya: What Works, What Doesn't, and Why. By Barbara Grosh. Boulder: Lynne Rienner, 1991. 223p. $32.00.

African Successes: Four Public Managers of Kenyan Rural Development. By David K. Leonard. Berkeley: University of California Press, 1991. 406p. $55.00 cloth, $18.95 paper.

Kenya has long been considered a development success story on a continent where most other countries have recorded negative economic growth and significant cutbacks in social development programs. Using a cross-section of economic and social indicators listed in the annual *World Development Report* (published by the World Bank) as the measuring rod, Kenya comes out among the top five African countries in almost all instances. More recently, however, questions have been raised about the solidity of the Kenyan successes. The increasingly autocratic and corrupt nature of Kenyan politics under President Moi has left many admirers of the country's proud development record in doubt about the prospects of keeping it alive in the 1990s. In late 1991, Kenya's principal donors took the unusual step of freezing their development aid for six months or until they see proof that President Moi has introduced democratic reforms and curbed the widespread corruption in the public sector.

The two books under review here help us understand many of the reasons for Kenya's progress but also draw our attention to how arbitrary and ill-conceived decision rules have increasingly come to threaten further advancement. Both authors assume that the human factor is important in development and provide convincing evidence that what public managers do makes a difference. This emphasis on agency, rather than structure, is refreshing and provides a useful counterpoint to the overwhelmingly structuralist interpretations that have for long dominated the literature on Kenya and other African countries.

Barbara Grosh's study covers the operations of 32 public enterprises over a 25-year period (1963–88). The sample contains almost all the firms that depend on revenue from sales and that are overwhelmingly state-owned. It includes enterprises serving agriculture, finance, and development finance institutions, as well as parastatal enterprises providing infrastructure services.

The principal conclusion of her study is that public enterprises in Kenya work remarkably well. She questions, therefore, the validity of the prevailing opinion in the Kenya government, as well as among donors, that they constitute a major problem. In her view, there is cause for neither extensive privatization (as the donors insist there is) nor greater political control (as the Kenya government believes is necessary). To be sure, not all enterprises operate equally well; but the reason for poor performance is typically external, not internal, to the enterprise. A main culprit is the government itself, by setting prices or insisting on decision rules that leave management without a chance to run the enterprise in the black. Kenya's parastatal managers, therefore, are not villains but victims of circumstances beyond their control. With greater sensitivity to the commercial needs of these enterprises, the Kenya government could provide them with a much more enabling environment.

Grosh has no major theoretical pretensions in her study. It is a down-to-earth examination of a long series of financial records. Its principal value is twofold. The prevalence of public enterprises in African countries notwithstanding, few independent studies have been done of these institutions. To be sure, there have been several public inquiries and reviews; but largely because of difficulty of access to relevant records, only a small number of academics have tried their luck in this field. Thus, Barbara Grosh's study is filling a vacuum. Second, her pioneering effort is enhanced by the comprehensive and longitudinal nature of the investigation. The result is a more nuanced picture of these enterprises and a greater appreciation of the many factors that shape their performance.

The flip side of the extensive examination of performance records is, of course, that the author has no time to carry out any in-depth study of the many cases she includes: statistics are speaking for themselves. One wishes that Grosh had provided her readers with more feeling for the reliability of the records she examines. Another criticism I have is that she fails to recognize that the conclusion she draws about the variable performance of these enterprises and the constraints placed on "heroic" managers echoes concerns expressed by these managers for over two decades now without any effective reforms having been taken. For example, a closer examination of the published records of the seminars held in the 1960s and 1970s under the auspices of the African Association of Public Administration and Management and its precursor, the Inter-African Seminar of Public Administration, would have placed the analysis in this study in a better historical perspective.

Fifteen years ago, Leonard made a major contribution to the field of development administration with his *Reaching the Peasant Farmer*, a systematic study of the agricultural extension service in Kenya's Western Province, using hypotheses derived from organization theory. With *African Successes*, he confirms his position as one of the leading scholars in this field. This study, however, is very different from his first one and contrasts also with that of Grosh. Using the life stories of four exceptionally successful public managers, Leonard explores the reasons for Kenya's positive development record since independence. Thus, he is this time more concerned with generating, rather than testing, hypotheses. This inductive approach leads him to two major conclusions: (1) that Kenya's advance can be attributed to the emergence of a class of independently wealthy people based in agriculture, rather than in urban-based enterprises, and (2) that professionalism makes a difference and needs to be carefully nurtured.

Leonard's book is not your usual political science text.

Though bold and imaginative, his methodology in the end earns him a lot of empirical mileage. With the help of some three hundred intensive interviews (some lasting almost a whole day) with the four principal personalities of this study and their relatives, friends, and former colleagues, he is able to provide a unique and fascinating insight into how policies were made in Kenya and what factors seem to have shaped the evolution of its politics. In weaving together the innumerable threads that his study engenders, Leonard moves in a masterful manner back and forth between the micro and macro levels of his analysis.

The author never gets lost in his rich empirical material. The book is very well organized. A principal set of arguments is consistently pursued. To make his story as readable and accessible to as broad a readership as possible (a gesture that few academics make), Leonard relegates his quantitative analyses to two appendixes. The result is that when you come to the end of the book you realize that you have a human interest story and an academic treatise in one, perfectly merged. This is a mark of outstanding scholarship, rare among even the more experienced and meticulous.

There is so much about this unusual book that one would wish to discuss, but suffice it to say that it is of interest not only to scholars in development administration. It makes a contribution in its own right to the study of Kenyan politics and also, albeit more distantly, to broader concerns of comparative politics at large. One also wishes that as many political scientists as possible get a chance to become familiar with his methodology, including his choice of emphasis on "deviant cases."

The latter is obviously not uncontroversial. What conclusions can one really draw from, in this case, the lifetime achievements of four public managers who stand out as different by virtue of their professionalism and leadership abilities? Leonard may be accused of overdramatizing the significance of his conclusions, particularly as they relate to the managerial significance of "pockets of professionalism" in the contemporary African developmental context.

This and other criticisms notwithstanding, Leonard's book must be counted as one of the most thorough and best-crafted studies by an Africanist political scientist in recent years. What is more, its significance and value transcends the boundaries of African studies.

University of Florida GORAN HYDEN

Free to be Foolish: Politics and Health Promotion in the United States and Great Britain. By Howard M. Leichter. Princeton: Princeton University Press, 1991. 281p. $35.00.

Doctors and the State: The Politics of Health Care in France and the United States. By David Wilsford. Durham, NC: Duke University Press, 1991. 355p. $49.95 cloth, $19.95 paper.

Whenever governments intervene in a policy area, they encounter complex webs of technical, economic, and political interrelationships. All these factors are present at an extraordinary level in the health policy arena. Governments confront well-organized and powerful interest groups of health care providers. They must also confront powerful funding agents for medical care. The politics are no less intense when government itself is the principal funder of medical care. Increasingly, government also confronts interest groups attempting to redefine the very nature of health care and to redirect the attention of well-structured, functioning policy communities. Understanding these relationships between state and society is important within any one country; but to conduct the analysis comparatively presents a much greater opportunity for developing political theory, as well as generating more real substantive knowledge about the policy area.

The two books reviewed here make significant contributions toward an enhanced understanding of the changing role of government in health care. David Wilsford's *Doctors and the State* discusses the more conventional question of the confrontation between the power of the state and that of the organized medical professions. Howard Leichter's *Free To Be Foolish* addresses the less frequently discussed issues of the role of the state in health promotion and the use of its power to make individual citizens behave differently to improve their own health. While the subject matter may appear somewhat different, the fundamental questions raised by the two volumes are really very much the same. One question is how the state can exercise its power effectively to improve the health of its citizens. The other question is the normative one of the appropriate role of government in altering behavior of both providers and consumers. Does the financial power, combined with its substantive knowledge, justify government's substituting its collective judgement about health for that of individuals?

My saying that the Wilsford book addresses a rather conventional question is not intended to denigrate this volume. The question is a familiar one, but this is an excellent treatment of the subject of the relationship between government and the health professions. The discussion extends the usual arguments to examine a range of connections between state and society. The selection of the United States and France as the two cases appears to make the question of state power versus professional providers too obvious for the researcher and reader; and to some extent it does. However, the author pursues the implications of the differential power of the state in interesting ways and also points out many of the subtleties of the two political systems. This is especially true in the case of France. In particular, Wilsford points out that the (relatively) strong and autonomous state in France is not omnipotent and cannot guarantee policy success, as the characterization of the "continuity of crisis" in French health care indicates. In addition, the two-country comparison is extended to include four other systems with varying degrees of government involvement in the provision of health care. This extended comparison helps to clarify the author's findings for the two major cases.

As noted, Howard Leichter addresses a somewhat more novel question of the role of the state in modifying individuals' behavior that can affect their health. It is hardly news that a great deal of an individual's health and well-being is a function of life-style. The question is whether government should encourage or require people to change unhealthy life-styles. Not only can government be seen to have some moral responsibility to improve the health of its citizens, it has a real financial stake as a major funder of health care (even in the United States) to reduce illness. These claims must be set against the claims of individuals for personal autonomy and individual freedom. These conflicting claims have very direct life-and-death importance in health care; but analogous conflicts can be pointed to in other policy areas in which government can be argued to be producing a merit good (e.g. education or the environment).

Leichter also bases his analysis of health policy on the comparison of two countries: the United States and the United Kingdom. He discusses an interesting and significant range of health issues, ranging from smoking to AIDS. Here, the findings are somewhat different from those in Wilsford's book, with the United States' being somewhat more interventionist and the British government tending to rely more on voluntary agreements. In each case, however, the effects of this "second public health revolution" is to shift much of the blame for poor health onto individuals and their own behavior, leaving government only the option of altering individual lifestyles if they are to be successful in promoting health. That leaves unanswered, however, the more basic question of whether governments should be in the business of promoting health, especially when it requires intervention into personal liberty. Leichter is to be complimented for his manner of raising these thorny questions and willingness to accept indefinite and partial answers to them. The analysis might have been improved, however, by some greater attention to issues of agenda setting and to how policy issues are socially and politically constructed.

These two books, taken together, are a useful introduction to the health policy issues facing governments in the 1990s. On the one hand, governments must make difficult economic decisions about funding health care and their willingness to use their powers to combat the powers of private health providers who can profit from public programs. On the other hand, governments must think about how to make citizens healthier as another means of reducing their costs of health care. The two strategies of intervention are complementary, and both involve substantial political costs. Readers will understand both the strategies and the costs better by having read these books.

University of Pittsburgh B. Guy Peters

Communities of Grain: Rural Rebellion in Comparative Perspective. By Victor V. Magagna. Ithaca: Cornell University Press, 1991. 277p. $42.50.

Serious study of peasant rebellion is a relatively new phenomenon; but the field has been greatly enriched in recent years by the work of Migdal, Paige, Popkin, Scott, Tutino, Wolf, and others. Magagna's volume seeks to make a contribution to this growing body of literature by proposing a different framework to answer the question, Why do peasants rebel?

The central thesis of this work is that agricultural societies cannot be understood in terms of class (as Marxists theorists argue) or in terms of subsistence (as those coming from the moral economy tradition would have it). Moreover, historical sociologists, such as Barrington Moore, Jr. and Theda Skocpol, who have analyzed such societies primarily from the point of view of nonagricultural elites and states are also in error. Such analyses have been faulty, it is argued, because they have led to ambiguous conclusions by an undue emphasis on economic issues.

According to Magagna, the most appropriate way to understand rural rebellion is to view the problem from the perspective of community, specifically "communities of grain." He acknowledges that the use of the community as the unit of analysis is not new, since it has been the staple of ethnographers for many years. But Magagna seeks to tighten the definition of community by focusing it on territoriality and land tenure and arguing that bucolic notions of egalitarian communities are largely inaccurate, since most communities are characterized by inequality and hierarchy, based on such factors as status, gender, and age. Despite these inequalities, they are offset by reciprocity and widely accepted rules that draw together communal residents, thereby allowing them to undertake mobilized political action. It is out of this community action that rural rebellion becomes possible.

Although there are many specific causes of rural rebellion, including conflict over land, religion, taxation, and so on, Magagna argues that no one of these can really explain the phenomenon. Neither relative, nor absolute, deprivation sparks such rebellions. Rather, such insurrections are a form of a broader phenomenon that he calls *representative violence*—an unfortunate use of terminology that will end up confusing many readers. He rejects the clearer term *bargaining by riot* for he fears its use might lead to confusion with urban movements. What is meant is that rural rebellion is to be interpreted as a means for communities to present (or re-present), in a violent fashion, their demands for a change in elite behavior. These movements, then, are a mechanism of community defense.

Communities vary along two closely related dimensions: autonomy and cohesion. This framework gives rise to three types of communities: redistributive, regulative, and residual. The case studies used in the book to illustrate these various types are compiled from cases that meet two criteria. First, they must be cases for which an extensive secondary literature has been compiled, a factor that results in the bulk of the cases being selected from Europe. Second, the cases must be central to the debate over the historical sociology of rural rebellion. These criteria motivate Magagna to select cases from France, Spain, and Russia. He adds England and Japan to the list as control groups—England because the individualistic tradition found there presents the most difficult test for a theory based on community and Japan so as to include a non-Western case. The illustrations are, for the most part, pre-twentieth century. The focus of the chapter on England is the period 1500–1800, that on France covers the period 1500–1900, that on Japan, 1600–1868. Those on Spain and Russia cover more modern periods, 1800–1939 and 1800–1930, respectively; but none of the material deals with the contemporary era. Each country is covered in a separate chapter, and extensive use is made of an impressive array of secondary materials, all carefully footnoted to enhance its accessibility to the reader.

The review of the cases produces two broad categories of rural rebellion: movements of secession and movements of reconstitution. The Russian and Japanese cases are an illustration of the former process, whereas the illustrations from England, France, and Spain are seen as efforts of communities to alter the nature of the relationship between local community and external elites and markets. The type of rebellion is not a function of macro societal factors, such as the level of modernization or commercialization. Rather, when commercialization occurs from above and becomes "coercive commercialization," secession is most likely to result, whereas, when it comes from below, a rebellion demanding reconstitution is the likely outcome.

The concluding chapter of the volume attempts to draw together all of this material via the presentation of nine hypotheses. Magagna admits that the hypotheses are not operationalized; but, he says, "The application of these hypotheses, even to third-world countries, may help to explain much about the nature of rural politics" (p. 256). He goes on to argue that the communities-of-grain concept is applicable, at a minimum, to Mexico and the Andean countries and portions of sub-Saharan Africa and Southeast Asia. One group of hypotheses deals with the question of the stability of communities; a second, with differences in levels of violence; and a third, with the variation in the impact of ritual on community cohesion.

The hypotheses presented will disappoint those who are looking for a theory of peasant revolution. Indeed, Magagna argues specifically that the rural rebellions he has studied, "do not add up to a revolution in the classic sense" (p. 258). Moreover, Magagna acknowledges that the enormous power of modern armies makes the rural rebellion of the type dealt with in this book increasingly obsolete. Yet he suggests that Vietnam may be a case in which rural rebellion is still an active force today, although few readers would agree that this is an instance of local community violence. In the end, he is compelled to admit that rural rebellion may now be "out of date" (p. 263), so that his study appears to be of little utility to those who wish to understand such important contemporary cases of rural rebellion as those found in Guatemala, El Salvador, Peru, and elsewhere.

This is a book with a large scope, both theoretically and substantively. It is an excellent illustration of comparative analysis; and the reader cannot fail to be impressed by the richness of its treatment and the effort to synthesize greatly divergent case material into a parsimonious set of nine hypotheses. The ultimate utility of the volume, however, will depend on scholarly acceptance of the notion of "communities of grain" as the appropriate organizing construct for understanding and explaining rural rebellion.

University of Pittsburgh MITCHELL SELIGSON

Militarism and Politics in Latin America: Peru from Sanchez Cerro to Sendero Luminoso. By Daniel M. Masterson. Westport: Greenwood, 1991. 345p. $47.95.

Daniel Masterson has done Latin American (especially Andean) specialists a considerable favor with this book. By concentrating on one country (Peru) and one fairly short historical period (60 years), he has produced a monograph that in extraordinary detail describes the decades-long search for what Masterson calls the Peruvian military's professional mission or rationale—in a word, its raison d'être. All militaries everywhere have two basic missions: to protect the nation-state from external aggression and to assure domestic order. But Third World militaries may not be threatened by outside aggressors or may play only a peripheral part in global power struggles. For example, in the twentieth century, Latin America did not have a major role in hemispheric defense, because the United States assumed such responsibility throughout the Cold War. Latin American militaries have thus found themselves doing either or both of the following: (1) protecting borders that (with a few notable exceptions) were not about to be invaded from the outside (which did not prevent arms races and very large military budgets), and (2) concerning themselves with internal affairs. The question that occurs, of course, is, What shape or form should such concerns take? The search for answers to this question comprises the core of Masterson's book.

Peru has for many years had one of Latin America's more idiosyncratic militaries. It has intervened in the political arena to such an extent that a constitutional transfer of presidential power has occurred only three times since World War I (1945, 1985, and 1990). Yet these interventions have not taken place simply because one caudillo wished to take the place of another. One of the strengths of Masterson's book is his ability to point out the multiple, often overlapping, and even contradictory motives behind the military's coups. Masterson traces the influences first of France and later of the United States, as countries that dominated Peruvian military training and describes the schizophrenic reaction as Peru switched from one to the other. But Masterson also takes care to show how domestic political circumstances in Peru affected the military. Conventional wisdom, for instance, has long held that the military intervened throughout the 1930s and 1940s primarily because of its deep-seated enmity for (APRA), Peru's then radical party. But Masterson demonstrates that such an explanation is simplistic by stressing the divisions within the military about APRA and the degree to which young, reform-minded officers often sided with APRA in planning uprisings and coups against oligarchic rulers who were supported by senior military officers. He also convincingly argues that interservice rivalries based on ideological and personal differences also played major roles, especially during the 1950s and 1960s and notes, as well, that the United States' insistence on anticommunist strategic planning and counterinsurgency techniques also played a role, as did the military's self-perception during the late 1960s as the only organization within Peru able to take on the reforms needed for independent development to take place. All of these arguments have been made previously, of course, but Masterson is especially successful in showing how these several themes become differentially interwoven over time and how they influenced the military's activities. Such success is possible thanks to his use of a wide variety of primary and secondary sources, including archival materials in Peru, as well as the United States, Peruvian military journals, and personal interviews.

Masterson's intent—to detail the Peruvian military's search for a clear professional rationale—is thus accomplished. For political scientists searching for an understanding of Peru and its military within some larger, more generalizable context of civil–military relations, however, the book is less satisfactory. No mention or use is made, for example, of Stepan's work on Latin American militaries or of the 1980s literature on transitions to democracy (O'Donnell and Schmitter; Diamond, Linz, and Lipset, etc.) to understand Peru's own transition in 1980. Social scientists attempting to fit the Peruvian case into a larger explanatory framework will therefore be left to their own devices. Since Masterson is, in fact, a historian, perhaps such a complaint is unjustified;

nevertheless, the main title should perhaps be dropped, since the book addresses Latin America in general not at all. That being said, Masterson allows Andean specialists to fill in numerous gaps in their knowledge and Latin Americanists to gain a much greater comprehension of Peru and its military than was previously available. How Peru can be best analysed and explained by a more general framework, or how such a framework might have to be modified to take account of the idiosyncracies of the Peruvian case, remains to be done.

University of Texas, Austin HENRY DIETZ

Partial Visions: Culture and Politics in Britain, Canada, and the United States. By Richard M. Merelman. Madison: University of Wisconsin Press, 1991. 287p. $40.00 cloth, $15.95 paper.

This is an ambitious, highly innovative, and partially successful work. Richard Merelman has produced an important and idiosyncratic study of popular culture and its relationship to democracy in the United States, Canada, and Britain. Though the back cover of the book hails it as a "pathbreaking study of political culture," the author is careful to distinguish his approach from "the dominant mainstream political science approach to the study of political culture," which is fraught with "insufficiencies," has "yielded few fruits," and consequently is a methodological antimodel, an example of "how not to proceed" (pp. 39–40, 44).

Though Merelman shares with Almond and Verba a deep concern for the cultural ecology of democracy, he insists that their approach, which relies on survey data and analysis of political attitudes, must be placed "within a broader theoretical framework" derived in part from anthropology (p. 44). While recognizing that there are several different anthropological schools of thought about culture, Merelman opts for the "symbolic" approach, which sees "culture as a system of discourse" and contends that "social behavior proceeds from and acts out symbolic systems, texts, or . . . *collective representations*" (p. 45).

Merelman undertakes to study cross-nationally collective representations of four collectivities (the *family*, the *corporation*, the *polity*, and the *world of consumption*) and their corresponding social roles (*family member*, *employee*, *citizen*, and *consumer*). Each of these collective representations become infused into the "system of discourse" of the "popular culture." Each therefore affects the understanding and practice of democracy, whose essential qualities, Merelman insists, entail a healthy amount of "conflictive democratic participation." He summarizes his argument by stating that "certain qualities of a society's popular culture subtly prepare people either to seek out political participation and welcome group conflict or to resist political participation and to reject group conflict" (p. 8).

In order to analyze these cultural tendencies in each of four collectivities, Merelman turns to the source of what he believes is the most influential collective representation of their corresponding social roles: television programs (family member), corporate publications (employee), high school textbooks (citizen), and magazine advertisements (consumer). Using qualitative analysis of these primary sources, supplemented by contextualizations drawn from selected secondary sources, Merelman provides us with interpretative essays on the unique "cultural configurations" of each of the three countries. This is followed by a chapter entitled "The Political Embodiment of Liberal Democratic Culture," in which he analyses voting behavior, public opinion, and elite political discourse, which are "surface embodiments" of the "deep structures" that comprise the cultural configuration. By far the most interesting aspect of this analysis is the quantitative and qualitative examination of political language in each of the three societies. Merelman presents paired comparisons of debates in Britain and the United States on party reform and texts of important documents on bilingual education in Canada and the United States. Like his use of the primary sources that underlie his characterization of the cultural configurations in the three countries, these analyses are methodologically innovative and enlightening. They do not, however, link up very well to his hypotheses; nor do they provide convincing elaboration of his definition of democracy as implying conflictive participation.

In a final, reflexive chapter, Merelman looks back over his work which he succinctly indicates is a "partial argument" and criticizes its limited validity. Merelman accepts that the problem of the intelligibility of culture, its relationship to institutions, and the problem of cultural dynamics and political change are not taken up thoroughly in his book. But he never extends his self-criticism to his characterization of democracy itself, which remains idiosyncratic and incomplete.

Despite its shortcomings, this book is, nevertheless, an important path-breaking study that should open the way to new research directions and inspire much controversy in the discipline. Merelman has explored new ways of conceptualizing culture and displayed exciting examples of qualitative and quantitative empirical analysis. Although it is perched uncomfortably on the conjuncture of positivist and postpositivist methodologies, it has much to contribute to the renaissance of interest in culturalist approaches.

York University, Toronto DAVID V. J. BELL

Social Currents in Eastern Europe: The Sources and Meaning of the Great Transformation. By Sabrina P. Ramet. Durham, NC: Duke University Press, 1991. 434p. $49.95 cloth, $19.95 paper.

This lengthy and ambitious volume seeks to describe and explain the social roots of the collapse of the socialist states in Eastern Europe in 1989. The author boldly includes all the countries of the region, although the coverage of Poland, East Germany, and Yugoslavia is more thorough than that of the remaining countries. The final chapter offers a hasty summary of political developments in 1989 and 1990, but the main focus of the study is on independent social groups and movements in the 1970s and 1980s.

The author's argument is that the popular basis of the East European regimes was steadily eroded by a broad variety of social processes, so that by the late 1980s, the arrival of Gorbachev could serve as a catalyst sufficient to trigger their collapse. In the abstract, one can hardly disagree with such a thesis; but the author does not offer a more probing analysis of the precise links between the social phenomena she describes and the regime changes that took place in 1988–89.

The social processes covered in the book are a fairly eclectic selection: Solidarity in Poland, dissident intellectuals in East Germany and Czechoslovakia, Yugoslav feminists, rock musicians, and churches and sects throughout the region. The author states that these are all manifestations of a process of "modernization"; but apart from some rather mechanical references to the works of Samuel Huntington and Crane Brinton, the analytical framework is not really elaborated and explained. For example, there is no systematic discussion of social mobility, urbanization, or education levels, all usually regarded as central to modernization theory. Most surprising of all, there is virtually no discussion of the role of nationalism, apart from a case study of the Albanians of Kosovo, Yugoslavia (Chapter 7). Developments since 1989 have, of course, clearly shown that nationalism has the capacity to displace group identity in several, if not most, countries of the region.

Thus, the value of the book is that it provides a selection of detailed case studies of social phenomena of East Europe in the 1970s and 1980s. Although the book is primarily derived from journalistic sources, including the invaluable Radio Free Europe reports, the material is so rich that even area specialists will discover something new in this volume.

Duke University PETER RUTLAND

Crisis and Choice in European Social Democracy. By Fritz W. Scharpf. Ithaca: Cornell University Press, 1991. 303p. $49.95 cloth, $17.95 paper.

This absorbing study, first published in German in 1987 and now appearing in a clear and elegant English translation, exemplifies two current trends in comparative political research. The first is the move away from general, inclusive theories toward more nuanced and theoretically more modest treatments that take account of differing national and institutional circumstances. The second is the renewed interest and concern with the impact of institutions themselves and the correspondingly greater emphasis on what is conditional and contingent.

Scharpf's subject is the question of political control of the economy. His data concern the experiences of four European countries (Austria, West Germany, Sweden, and the United Kingdom) during the economic recession of the 1970s and, more specifically, the differing policy responses of the then social democratic governments of these countries to that crisis. Rejecting any notion of "crisis as fate," Scharpf points out that while there was a general worsening in the indicators of economic performance throughout Western Europe in the early 1970s, variation between the subsequent performances of the different countries also increased. His goal, then, is to account for this variation by selecting a group of countries governed by essentially similar social democratic governments that found themselves, at least initially, in similar economic circumstances. This "most similar systems" design enables him to assume a communality in the goals of each of the four governments and to assume, in particular, a shared commitment to full employment. This also allows him to assume that the subsequent variation in performance did not derive from differences in policy priorities. Rather, and of necessity, this variation must have derived from the differences in the capacity to govern and the ability to steer the economy through the crisis, which, in turn, depended at least in part on the differing institutional circumstances of each government.

This is the thesis that Scharpf sets out to argue in his overall analysis. His results are very convincing, and the end product is a genuinely excellent piece of work. Theoretically sophisticated, empirically grounded, and closely and persuasively reasoned, this book is a powerful example of how good comparative political analysis can be; and it constitutes one of the most important contributions to the rapidly growing literature on the political economy of contemporary Europe. We learn that politics and governments matter and that institutional structures are more than simply neutral arenas but have a capacity to condition and facilitate particular policy responses, while discouraging others. And along the way, we gain a valuable insight into the dynamics and constraints of macroeconomic policymaking, as well as a clear understanding of the parameters of the relations between trade unions and governments. There is, in short, a wealth of wisdom here, extending far beyond the ostensible limits of a study of four countries during a relatively short and distant period of time.

What also becomes clear from this study (albeit less directly) is that while governments in general matter a lot, the sheer importance of institutional structures also means that any individual government does not matter so much. This becomes evident when Scharpf addresses two of the key questions set by his study: under what institutional conditions unions will opt for a politically motivated strategy of conciliation with government, rather than an economically motivated strategy of confrontation and under what conditions governments can actually act on the basis of their chosen policies (pp. 169–237). In both cases, it seems, the die was pretty well cast well beforehand. In the case of union strategy, confrontation tended to be avoided in circumstances where there already was an established tradition of employer–labor cooperation and union–government cooperation and where there existed a cohesive union organizational structure. In the case of the government, the governing capacity itself tended to be enhanced when central government had direct control of the relevant monetary and fiscal instruments. Yet both sets of circumstances are clearly long-term legacies, quite beyond the scope of any government to alter substantially in the short term. In this sense, the fact that the Austrian social democratic governments managed to weather the crisis more effectively than their British counterparts may have more to do with their being Austrians than with their being better social democrats.

Scharpf concludes his study with an insightful account of the changing international economic circumstances of the 1980s and an analysis of the difficulties that confront social democratic ideologues in the new, non-Keynsian world: "Ideologically, and in public debate, at any rate, the hour of supply-side policies has always been the hour of employers and conservative parties" (p. 270). The continuing inability of contemporary social democrats to elaborate a convincing program that can tackle these new circumstances in the interests of their traditional constituencies clearly bodes ill for the future of the movement as a whole.

University of Leiden PETER MAIR

Radicalism and the Origins of the Vietnamese Revolution. By Hue-Tam Ho Tai. Cambridge: Harvard University Press, 1992. 325p. $34.95.

The Vietnamese have a profound consciousness of history—are, in fact, burdened by it (unlike the Americans). And, as their late prime minister Pham Van Dong once observed, they are incurable romantics about politics. Both of these cultural characteristics are amply demonstrated in this study of the principles, ideals, and operational methods of Vietnam's young radicals of the 1920s. The author is a leading ethnic Vietnamese woman in the American academy. Her field is twentieth-century Vietnamese intellectual thought, which locates her in that grey area where history overlaps political science.

This work has a distinct biographical orientation to it, a roll call beginning with the godfathers of the Vietnamese revolutionary movement (members of the turn-of-the-century Scholars movement who launched the intellectual revolt against traditionalism and French colonialism) and running through the nationalist and proto-Marxist figures of the next three decades. Tai's treatment is in the spirit of Woodrow Wilson's contention that history is but the lengthened shadow of great men. (Presumably, today he would add, "and great women"). A plethora of names appears here, although the radicals probably numbered less than two thousand of what today would be called dedicated activists led by not more than 250 of what today would be called cadres. Only a few of these are familiar to even reasonably well informed outsiders. Few survived to take a place in the history of the revolution that was to come, even as footnote.

It must have been an exhilarating time for these young political groundbreakers. And young they were, few past their midtwenties (Ho Chi Minh, in his early thirties, was their grand old man of revolution). They debated, plotted, published, organized, experimented with politics. They split into factions, regional groupings, and personal entourages. Most were elitist. Theirs was a cat-and-mouse game played with the French, encouraged by supporters in metropolitan France, and also, a darker game against the malignant French Indochina *colon*.

Their heritage was traditionalism with its strong Confucian, Buddhist, and Taoist influences that had created such a disciplined Spartan-like people; the millennia-long China threat of *han-hwa* (sinoization); a singular style of politics of clandestinism, whereby the Vietnamese dealt first with the Chinese, then with the French—all of which meant that by the 1920s, Vietnam was an ideal environment for political conspiracy.

The book is divided into eight chapters, treating the Confucian–French educational system, which shaped the young radicals and created the initial streams of revolutionary thought, more anarchial than anything else; the female radicals and feminization of the cause, about which virtually nothing has been written; the scandal sheets where young editors thundered against the relentless corruption of French colonialism; and three final chapters about the radical cause's taking on true political coherence.

Initially this contention pitted the nationalists (chiefly the Viet Nam Quoc Dan Dang, patterned after the Kuo Min Tang of China) against a proto-Marxist youth group, Thanh Nien, which metamorphosed into Trotskyist and Stalinist groups in the 1930s, finally to be captured by the latter. All streams were militantly anticolonial; but the Stalinists considered the others mere bourgeois revolutionaries, individualistic, non- or anticollectivist, and unwilling or unable to break completely with past value systems.

A common perception of academics viewing Vietnamese history, particularly those who come to it from a China background, is that accidents of history cheated Vietnam out of a vigorous intellectual tradition with national focus; that the literate class in Vietnam was always far smaller proportionally than in China; and that romanization of the Vietnamese language and other factors reduced Vietnamese access to China in ways that it did not in Japan and Korea. It is precisely this perspective that Tai seeks to challenge. Her second major thesis (as I read it) is that the spirit of the Vietnamese revolution was not chiefly a matter of borrowed westernism but was authentically Vietnamese, rooted deep in its culture. A third point is that this early radicalism was very much a young people's enterprise and, to a large extent, a young Vietnamese women's enterprise. (For those of us who have lived long in Vietnam this work reminds us just how really tough, mentally and physically, are Vietnam's women).

My chief quarrel with Professor Tai is that she tends to treat the Vietnamese revolution—indeed, Vietnam's political history in general—as a coherent entity and sharp-edged, as a clear progression of events, when, it seems to me, it is a great muddy river of events full of backwaters and eddies. The Vietnamese revolution, in particular, I see as a messy shaking out of history whose meaning is still being sorted out. Tai also seems excessively harsh on Confucianism, which is, after all, one of the great intellectual streams of thought: it produced a governing system that lasted longer than any other, save Egypt.

This is a bittersweet book. One puts it down with feelings of admiration for those young Vietnamese radicals of the 1920s but also with a sense of tragic waste. There were then (it is clear now) many roads to revolution in Vietnam. Almost any one of them would have been better than the one taken. Perhaps it was inevitable, as Tai seems to conclude. In any event, this is a fine book, commended to all.

University of California, Berkeley DOUGLAS PIKE

INTERNATIONAL RELATIONS

The Water's Edge and Beyond: Defining the Limits to Domestic Influence on United States Middle East Policy. By Mitchell Geoffrey Bard. New Brunswick: Transaction Books, 1991. 313p. $32.95.

A long list of books is available on U.S. policy toward Israel since its establishment in 1948. By several criteria, this one is distinctive. Most notably, perhaps, its approach to the subject is original and imaginative. The author applies several extant theories of the American foreign policy process to a series of case studies in Israeli–American relations. At the end of each, he evaluates the degree to which each theory is (or is not) useful in accounting for decisions taken by policymakers in Washington affecting the State of Israel. His approach, therefore, brings to bear an unusual combination of theoretical and eclectic insights in an investigation of the policymaking process.

Bard's study also stands out because he provides us with as objective and remarkably balanced an analysis of pro-Zionist lobbying activity and its impact upon American policy toward the Middle East as is possible for one of the most controversial and emotionally charged issues in American diplomacy. The author rejects the widely prevelant view that in most key respects, Washington's policy toward Israel has been "dictated" by the Zionist lobby. Time and again, he makes the point that a fundamental precondition for the lobby's success has been the underlying sympathy of the American people and their political leaders for the Israeli position—a factor that the Israeli government and its supporters in the United States have skillfully exploited. By contrast, this crucial reality has been lacking with respect to the "Arab lobby" (if, given its inherent disunity, this term can be meaningfully used to describe the Zionist lobby's main rival in the United States).

Another highly significant finding of Bard's investigation is that among several important variables contributing to the outcome of Zionist lobbying efforts, the ideological position of the president is crucial in determining the policy of the United States toward Israel. In his view, this is more influential than any other single factor. When, for example, the prior positions of the president and the pro-Zionist lobby happen to coincide, the latter can be expected to achieve a high level of success in attaining its stated goals. When they disagree, however, contrary to prevailing opinion about Zionist lobbying efforts, pro-Israeli groups are likely to have a relatively poor, or below-average, record in attaining their avowed goals. Bard also finds that as a rule, the Israeli cause tends to be supported more by Republican, than by Democratic, chief executives. In his view, this curious result can be accounted for largely by the fact that Democratic presidents and officials can and do take for granted widespread support for their party by Jewish voters.

Another distinctive contribution of Bard's study lies in focusing attention upon a phenomenon seldom dealt with in other investigations of the role of interest groups in the political process. This is the extent to which the president and his advisers often use pressure groups like the pro-Israel lobby to promote their own policy goals. Lobbying, Bard convincingly contends, involves something much more complex than merely a one-way attempt by interest groups to impose their views upon officials in Washington and public opinion. Much more frequently than even informed citizens of the foreign policy process are aware, for example, executive officials rely upon, or use, lobbies to achieve their own policy objectives.

Bard's study is clearly organized and interestingly written. The research relies upon both primary and secondary sources. Overall, he has given us an extremely valuable, informative, and provocative analysis of an important and recurrent issue in American foreign policy.

Louisiana State University CECIL V. CRABB, JR.

Command in Crisis: Four Case Studies. By Joseph F. Bouchard. New York: Columbia University Press, 1991. 325p. $45.00.

This is an important new addition to the literature on Cold War crisis decision making, even if such matters seem less pressing today in a world in which the chance of militarized disputes between great powers seems relatively remote. Bouchard is concerned with "the problems that can arise when using military force as a political instrument in crises" (p. ix). He focuses on a dependent variable of high policy relevance, namely, "the occurrence of inadvertant escalation in international crises" (pp. xiv, 188). What he really does in this book, however, is examine in detail the obverse side of the inadvertant escalation coin, that is, how the lid was kept on crises between the superpowers through various mechanisms of control.

In his effort to develop an explanation of this inadvertant escalation (or nonescalation) phenomenon, Bouchard adopts a comparative case study approach and examines the "interaction of military forces in crises and the impact of such interactions on crisis stability" (p. xiv). He selects four crises in which naval forces played a significant role: the 1958 Taiwan Strait Crisis, the 1962 Cuban Missile Crisis, and the 1967 and 1973 Arab–Israeli wars. Apart from this theoretical purpose in developing them, the case studies are useful summaries of events in these crises. The author also introduces new primary source material.

A significant limitation of the book is that it is difficult to generalize too far beyond these cases. Thus, one encounters problems extending the conclusions Bouchard draws from his detailed study of naval forces to ground, air, or joint forces that operate in very different contexts with different command-and-control mechanisms and levels of complexity. Moreover, his case studies are all about dyadic great power relations (i.e., U.S.–Soviet), so that his conclusions cannot be extended easily (and certainly not without caveat) to multipolar settings that also include small states (circumstances more characteristic of present-day world politics).

Bouchard observes that the U.S. Navy's institutional orientation is to put "peacetime missions" such as show of force for political purposes as secondary to "wartime missions." Nevertheless, all four case studies illustrate use of naval forces for "political signaling or related political functions" quite apart from their wartime use (p. xxxi). In fact, it is peacetime use of the navy short of war that is the more common use of force, which accounts for much of the success the U.S. Navy has had politically in getting budgets to sustain its "naval presence" and "power projection" capabilities.

The author's conclusion that crises can be controlled politically—that "tactical-level military interaction normally will not escalate to war without a deliberate decision by national leaders"—is reassuring, at least for the two great power confrontations he examines. Certainly, the situation grows more complex as the number of powers involved in a crisis increases, which makes maintenance of political control more difficult. Moreover, the stability that obtained in U.S.–Soviet crises was also a result of having "tacit rules of crisis behavior and communications" that had become "well developed" over time (pp. 198, 222). Whether even the relatively simple dyadic structure of conflict that governed U.S.–Soviet Cold War relations would necessarily be so stable without these norms is a central question.

The need for maintaining political control over military forces is never more important than it is in crises. Bouchard observes, "In an acute crisis, in which strategic or tactical interactions between the two sides have become decoupled from political-level interactions, the security dilemma can trigger an escalatory spiral at the strategic or tactical levels of interaction" (p. 198). Beyond tacit rules of crisis behavior and communications, escalation-inhibiting factors Bouchard observed in each case included military "prudence," caution and restraint by commanders, national leaders' structuring the tactical environment to dampen military interactions, accurate and timely tactical intelligence, and "double-checking the accuracy of initial reports of military incidents" (ibid.).

Although Bouchard acknowledges that they may not always hold, he nevertheless concludes that "these factors tend to moderate the intensity of tactical-level interactions, prevent armed clashes from occurring, and produce disengagement rather than escalation when clashes do occur" (p. 198). This is an important conclusion, so long as one keeps in mind the danger of extending it to multipolar and other settings not considered by the author. Seeing crisis stability in part as a function of relations between tactical and political levels on each side, as well as relations between adversaries, is an important insight Bouchard develops.

Notwithstanding the positive conclusions from the four cases he studies, Bouchard still maintains that naval forces "have serious crisis stability problems"; in a "severe" crisis, for example, he observes that "tension between crisis management and naval strategy objectives would be acute" (p. 196). Indeed, as is well known, President Kennedy recognized this problem explicitly as he and Secretary of Defense McNamara tried to maintain control of naval forces in their confrontation with Soviet counterparts during the Cuban Missile Crisis. Add to this imperfect information and the uncertainty or Clausewitzian "fog" that hangs over the battle area, and meaningful control by the political leadership is elusive: "National leaders must expect that at least some things will go wrong when they employ military forces in crises" (p. 221).

The case studies examine the relation between decision making at tactical and political levels in what the author refers to as "stratified interaction" (pp. 36–56, 222). Understanding crisis decision making as being "stratified," Bouchard observes that "direct control" by political leaders was only "exercised selectively and to a limited degree"; instead, political leaders relied primarily "on mechanisms of indirect control" such as predetermined terms of reference that specified limits to guide decision making by tactical commanders (p. 221). Bouchard goes beyond such observations to argue that national leaders should "focus on communicating with the other side in order to avoid misperception" and other miscommunication, rather than trying "to seize direct control of tactical decisions"; indeed, to Bouchard, "the greatest danger of a crisis escalating to war may well arise from decision making at the political level of interaction, rather than from decision making at the tactical level of interaction" (ibid.).

The effectiveness of indirect mechanisms of control in crises between the Cold War superpowers was heavily dependent upon the professional character of military commanders—seeing their political subordination as being legitimate. Such an assumption about all the militaries of great and lesser powers in the complex, multipolar world politics of coming decades is difficult to sustain.

In the final analysis, the author is careful not to overextend the important arguments he makes. His conclusions from the structured comparative case studies are really hypotheses worth testing or examining under the different conditions and contexts that will likely apply in coming decades. After all, the relative simplicity and stability of bipolar relations was something of an aberration from the historically more common multipolar mode. We need to know more about crisis decision making in the latter context. But that is not a fault of this book, which stands on its own merits.

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PAUL RICHARD VIOTTI

The Politics of Negotiation: America's Dealings with Allies, Adversaries, and Friends. By Linda P. Brady. Chapel Hill: University of North Carolina Press, 1991. 269p. $32.50 cloth, $12.95 paper.

Reviewers commonly choose a classic as a standard of comparison if they wish to point up shortcomings in a volume. I wish to use a classic—Fred Ikle's *How Nations Negotiate* (1964)—to identify the limited but, nonetheless, significant contributions made by Brady's monograph.

The longevity of Fred Ikle's masterpiece is attested by its continuing sales. Nevertheless, its concerns relate narrowly to the Cold War as seen from the perspective of the early 1960s. Scholars have welcomed several specialized but useful supplements. Linda Brady's book on how the United States negotiated military capabilities and operations in the 1980s is such a valuable supplement. It shares with Ikle's classic an admirable clarity of presentation and unwavering focus on a few, simply (but never simplistically) expressed factors and heuristics.

Brady's book is methodologically narrower than Ikle's. After an introduction, the book presents what is, in effect, a matrix. Each cell offers the author's answer to one question about one case. Six sets of questions range from the predictable (What factors influenced the formulation of negotiating positions?) to the vital but often neglected (What additional action was required for implementation of the agreement?). The negotiations treated are wartime host nation support with West Germany in the early 1980s; the Dual Track long-range theater nuclear forces deployment negotiations of 1979; Mutual and Balanced Force Reductions (MBFR) 1973–89; Intermediate-Range Nuclear Forces (INF) 1980–87; logistical support for the Persian Gulf in the early 1980s; and NATO out-of-area activities in the early 1980s. Since these vary significantly in duration, internal complexity, and relation to other negotiations, the reader would be helped had the author given more attention to her constitution of a "case."

Scholars of contemporary international negotiation will not find much new regarding negotiating theory. Nor will close observers of the public record of these cases often be surprised by the facts presented. The contribution of the book derives from meticulously comparative treatment, where variations of context and process are itemized, and from which are drawn some hypotheses about future negotiations.

The book is honest to its title: without reservation it is a discussion of politics. Brady ignores economic models, even when they might underscore her conclusions. For instance, she observes in the Preface that the most difficult negotiations occur between friends. This claim begs for a baseline of comparison: most difficult to conclude? most inefficient in process? most ineffective in resolving the problems that motivated negotiations? most unlikely to reach mutually beneficial and lasting agreements?

Scholars whose interests transcend strictly political factors might object to a few features of the volume's organization and to a few of its theses. For instance, Brady divides the six cases into three main types, said to be characterized by dealings with *allies*, *adversaries*, and *friends* and *mixed relationships*. There is another perspective which maintains that all negotiations that actually get under way must either be responding to their participants' recognitions of mixed motive situations or be motivated solely by the pursuit of side-effects (in Ikle's terms). Even within her carefully maintained focus on politics, Brady demonstrates a near aversion to economic phenomena and to the economist's methods. This is surprising because at several points she relates decision calculi that explicitly incorporate anticipations of other parties' responses, and she often relates participants' sophisticated assessments of trade-offs.

As an experienced Pentagon insider, Brady's own policy analysis and writing must have sensitized her to often-conflicting requirements for advocacy of policy, on the one hand, and its ex post justification, on the other. That same practical experience must have conveyed the often overdetermined "logics" by which policies emerge. Her first question about each case is, "What prompted the United States . . . to enter into negotiations?" Typically, narrowly rational, means–ends thinking is ascribed. What she seems to report is an answer to a subtly different question, namely, "What is the best case that could have been made for the United States to enter into negotiations?" The possibility that there were a multiplicity of converging pressures and complementary (if not entirely reinforcing) reasons is often overlooked. At several points in the treatment of the MBFR and INF talks, she appears to accept as unproblematic the existence of a "U.S. position" and makes observations rather casually about shifts in that position.

Ikle aimed his comprehensive analysis at the improvement of negotiating practice. Brady's final chapter, "Implications for Policy," is quite thin. I think that the case analyses plus the author's experience in the Office of the Secretary of Defense empower her to hazard far more in the way of "implications," even if they were modestly labeled "suggestions." In only one of Brady's cases is the U.S.–Soviet competition not central. But her analytic framework—succinctly conveyed as six sets of questions—is broadly applicable. The analysis is germane for understanding post–Cold War negotiations.

Syracuse University James P. Bennett

Learning in U. S. and Soviet Foreign Policy. Edited by George W. Breslauer and Philip E. Tetlock. Boulder: Westview, 1991. 881p. $75.00 cloth, $29.95 paper.

This timely collection defines *learning* as a rigorous concept and applies it to return engagements with similar issues in American and Soviet foreign policy across the past 45 years. It defines a new level for studies of decision-making, adding an historical dimension to the Allison tradition of analyzing single-decision cycles. It is likely to define key lines of investigation in international relations during the 1990s.

Karl Deutsch pioneered the study of learning with *The Nerves of Government* (1966). In the late 1970s the National Science Foundation supported an initiative for a new, interdisciplinary field along these lines. Now, with additional decades of data, it is possible to place Deutsch's inquiry on a solid evidentiary foundation. The framework will be especially helpful to study how political leaders respond to, and shape, new and changing international realities.

The volume represents a major investment, with 19 contributors and chapters of unusually high quality. This partly reflects its sponsorship by the National Academy of Sciences and foundation support that afforded a distinguished advisory committee, discussions among participants (beginning in the spring of 1987), and two editors of unusual ability and commitment who challenged their contributors to produce an integrated volume.

The volume is divided into four parts. An initial section discusses conceptual issues (contributions by Tetlock and Haas being especially notable); three concluding chapters partially integrate the volume. Between, two sections offer a well-planned series of case studies by experts in each field: seven on learning in American policy toward arms control, détente, Western Europe, China, the Middle East, Third World intervention, and nuclear nonproliferation since World War II; then a parallel series of studies of Soviet policy. Leading scholars have written the chapters, which include extensive references and provide sufficient space to develop complex themes.

The editors devote the conceptual chapters primarily to definitional issues, apparently in an effort to achieve rigor and clarity about the nature of their inquiry. Several disagreements emerge: approximately, the contributors divide into two camps. Some (like Tetlock) define learning broadly (and see more of it). Others (like Haas) would require an extraordinary set of changes, and substantial evidence, to infer that learning has occurred (i.e., as opposed to mere *adaptation*). As one of the editors (Breslauer) argues, a variety of definitions and different calibrations of thresholds probably matters little, so long as one is explicit about the dependent variable and processes under investigation.

But I would add a caution. Nonlearning (or mere adaptation) is not the same phenomenon as statistical randomness or an equivalent (an established baseline of governments); thus, Haas's null hypothesis is not a deep analysis. The sophisticated, surly, and cynical viewpoint that "governments don't learn (they merely *adapt*) because that is the *nature* of governments" would be as underspecified and unfortunate a causal explanation as Morgenthau's now-problematic claim that "objective laws" sustain the realist behavior of states.

Presenting definitions, Tetlock boldly argues that concepts of differentiation and integration from cognitive psychology are always correlates of policy learning. This hypothesis predicts that identical "Tetlock shifts" (beginner-to-expert) will occur in both international and domestic policies. When the editors conclude that both Soviet and American leaders "have learned that many indigenous processes in the Third World are out of the direct control of either superpower" (p. 13), one imagines a series of doctoral dissertations and books testing this theory against data drawn from domestic policy and implementation problems of the Great Society.

Can the theoretical apparatus of this book fully explain the end of the Cold War? Possibly not, because the authors' image of learning appears closer to learning in academic or scientific life. But by the beginning of the 1990s, one did not simply see reality more clearly, as a scientist. The nature of a socially constructed reality and its supporting emotional consensus changed for all participants—as if one were living inside a dream or a nightmare and awakened.

For example, viewing objective evidence, it was arguably crazier to have stopped the Cold War abruptly than to have started it. By the late 1980s, both sides had vast nuclear arsenals explicitly targeted at one another; 40 years of global conflict had shaped the relationship and given evidence to confim the hostile images of ideological formulae; the American president had called the Soviets an "evil empire" and was engaged in the largest arms race buildup in history; both sides were engaged in open support for proxy armies (Nicaragua, Afghanistan) in one another's spheres of influence. But in the late 1940s, against a background of wartime cooperation, no objective evidence of such magnitude and persuasiveness was available to justify the beginning of the Cold War.

Thus, if we contrast the two end points, was it not profoundly irrational and dangerous for the Soviet leaders to call off the Cold War? Not just to reach an arms control agreement (this might be predicted by realism and many other theories) but to dissolve the Warsaw Pact? To allow the breakup of the entire Soviet state? To welcome Western investors with open arms? Emotions can either block or support new learning; and one suspects that at a gut level, Soviet leaders decided to *trust* the West. And if so, this deeper intervening shift in emotional consensus, more consequential than the "objective" evidence of American arms buildups, is a causal mechanism whose nature, amidst the discussion of cognitive changes, remains a mystery.

With Tetlock's definition of differentiation and integration, the reader is prepared to read a dazzling tour de force in the last chapter, a crisp, refined, detailed, and brilliantly integrated outline of the causal variables and theories of the previous eight hundred pages. Yet, although Breslauer's chapter (and Weber's) are thoughtful and rigorous, they only begin to mine the volume. We do not know the box score of existing theories. It is left to future researchers (or graduate students) to inventory the new boxes, subboxes, and arrows implicit in this de facto update of Deutsch. The volume is a wonderful start—and by its own conception, the field has more work ahead.

University of Toronto LLOYD S. ETHEREDGE

Systems in Crisis: New Imperatives of High Politics at Century's End. By Charles F. Doran. New York: Cambridge University Press, 1991. 294p. $49.50.

Charles F. Doran has written a wide-ranging and incisive book that argues persuasively that power cycle theory is a potent analytic tool of analysis for understanding international relations in general and the outbreak of major war in particular. The book is ambitious, and the result something of a triumph. The author constructs a powerful case for claiming that the key to understanding the behavior of great powers (especially the sort of irrational behavior that commonly precedes cataclysmic war) is the trauma associated with widening gaps between national interests (or roles) and power (relative, not absolute), trauma which is the consequence of the power cycle. He argues that when a number of such traumas occur simultaneously, as they must owing to systemic interdependencies, the probability of major war increases dramatically.

Doran builds his argument meticulously. In part 1, he lays out the power cycle model—its essential concepts, their operationalization, and the data used to measure them—and places it clearly in its metatheoretical context. (There is also a brief but useful mathematical appendix). Although firmly embedded in the power politics tradition, power cycle theory (unlike balance of power) is dynamic and focuses on long-term change itself. One may quibble with the way Doran chooses to measure power: for example, the absence of contextual factors. But his combining of economic and military variables is a reasonable compromise that avoids the biases that flow from overemphasizing one or the other. The author recognizes the greater difficulty in precisely measuring international role, with all of its normative baggage, but his elaboration of this inherently slippery concept through historical and case description is entirely satisfactory.

Part 2 makes a deceptively simple (and highly effective) shift from the state and dyadic levels to system-level analysis. Doran shows how structural disequilibrium flows from a situation in which a number of states simultaneously experience "critical points" in their power cycles (such points being defined as changes in the slope or direction of the cycle that negate foreign policy expectations). The author's wedding of empirical data and historical analysis in making his case is unusually careful and illuminating. Indeed, his treatment of the causes of World Wars I and II illuminates geniune historical anomalies and shows the utility of political science for traditional historians.

Doran argues that such shifts are "perceived" by national leaders and publics, who are shocked by them and may respond in unpredictable ways. The link between structure and perception is necessary in order to avoid the charge of structural determinism and so prepare the reader for the prescriptive analysis to come. However, although the argument is plausible, there is tension between the claim that shifts are perceived and the central role played by surprise in the analysis. Moreover, the process by which perception takes place remains nebulous. Nevertheless, Doran ingeniously manages to open the national "black box" and to introduce and justify the splendid prescriptive essay that is to follow in part 3.

Parts 3 and 4 will assure that this book becomes required reading for graduate seminars in international relations theory. Having avoided the sort of frustrating determinism that characterizes long-cycle and hegemonic theories (which the author addresses), as well as the static bias of much of power theory, and having wisely ignored the putative "levels-of-analysis" problem, Doran examines at length the prerequisites of world order and the prospects for avoiding cataclysmic war. He then applies a similarly provocative analysis to contemporary world politics, especially to the implications of American and Soviet decline and Japanese and Chinese ascent (though from different points on their respective power cycles).

The examination of the recent past and future alternatives of the United States and the Soviet Union highlights the critical importance of linking system-level structural factors in order to make sense of international relations. Unfortunately, though imaginative, it also reveals the limits of power cycle theory in dealing with the domestic arena. These limits are especially evident in the (dated) discussion of probable future alternatives for the Soviet state and the causes of American behavior in Vietnam.

If Doran's effort to examine policy choices suffers from some of the drawbacks inherent in the power tradition, his willingness to tease normative implications concerning "justice" and "legitimate" behavior from the power cycle model constitutes an important contribution to the discipline. His claims regarding the relationship between power and justice and the obligations of leaders to design policies that will narrow status–power discrepancies in rising states are not uncontroversial. Nevertheless, the effort is an important and welcome step toward relinking empirical and normative analyses in international relations theory.

In short, whatever one's view of the statist and power politics tradition of which this book is a part, it is difficult not to be impressed and enthused by its breadth. It is among the most important theoretical and empirical additions to the causes-of-war literature of the past decade. More importantly, it suggests that there is life yet in what many observers believe is the increasingly moribund realist–neorealist tradition.

Iowa State University RICHARD W. MANSBACH

Moscow and the Middle East: Soviet Policy since the Invasion of Afghanistan. By Robert O. Freedman. New York: Cambridge University Press, 1991. 426p. $59.50.

Soviet Policy towards Syria since 1970. By Efraim Karsh. New York: St. Martin's, 1991. 235p. $59.95.

The USSR and Iraq: The Soviet Quest for Influence. By Oles M. Smolansky. Durham: Duke University Press, 1991. 347p. $55.00 cloth, $24.95 paper.

The political and economic changes associated with the tenure of Mikhail S. Gorbachev as president of the Soviet Union have had substantial impact on Soviet foreign policy. *New thinking* (as the reinterpretation of Soviet foreign interests and behavior is termed) has resulted in major arms control agreements and a substantial reduction of the Soviet military presence in Central Europe, the liberation of Soviet client–states in Eastern Europe, the withdrawal of Soviet troops from Afghanistan, a dramatic reduction in Soviet military support for erstwhile ally–clients throughout the Third World, and Soviet support for the U.S.-led military operation in the Persian Gulf. As this partial list indicates, Soviet policy in the Middle East has been affected by policy shifts as significant as those elsewhere throughout the Third World.

Three recently published book by established analysts of Soviet policy in the Middle East provide both background for, and an evaluation of, recent Soviet policy. Taken together, these books provide detailed coverage of key elements of Soviet policy throughout the region.

Over the past 20 years Robert O. Freedman has been among the most important and most prolific of Western writers on Soviet policy in the Middle East. His most recent *Moscow and the Middle East*, brings the story he first told in *Soviet Policy Toward the Middle East Since 1970* up to the Soviet withdrawal from Afghanistan in early 1989. As those familiar with his earlier writings would expect, his new study provides a detailed chronology of the development of Soviet policy in the key issue areas of importance throughout the region. Freedman weaves together a treatment of domestic developments within the Middle East, his interpretation of Soviet objectives, and the interaction of Soviet and other external actors to produce a comprehensive picture of Soviet policy toward the region during the 1980s. This book (like all of Freedman's work) is based on a wealth of documentation.

Freedman concludes that as of early 1989 when the Soviets withdrew their troops from Afghanistan, Gorbachev's policies toward the Middle East were characterized more by continuity than by dramatic changes associated with new thinking. The areas of continuity included (1) the Soviet plan for resolution of the Arab–Israeli conflict, (2) the Soviet unwillingness to use military force to challenge either Israeli or U.S. activities in the region, (3) the inability of the Soviet Union to effect reconciliation between Syria and the leaders of both the PLO and Iraq, (4) the policy of trying to balance between Iran and Iraq during the ongoing war, and (5) the Soviet disinformation campaign targeted against the United States (p. 323).

Freedman does find new elements of Soviet policy in the increased emphasis upon the role of the United Nations, the efforts to normalize relations with both Egypt and Israel, and the overall more flexible and active nature of Soviet policy in the region.

While Freedman provides the reader with a *tour d'horizon*, the other two authors provide detailed country studies. For almost two decades the Soviet relationship with Syria was a centerpiece in its overall policy toward the Middle East. In a perceptive analysis entitled *Soviet Policy Toward Syria since 1970*, Efraim Karsh examines the nature of Soviet policy toward Syria, including the basic objectives, the instruments employed in pursuing those objectives, and the costs and gains associated with that policy.

Karsh argues that the basic objective of Soviet policy remained essentially unchanged, namely, the establishment and maintenance of regional stability. Karsh begins by arguing that the Soviet–Syrian relationship has been neither one of patron and client nor one in which the smaller has been able to control the policies of the larger. He argues that although the Soviet Union has provided substantial military support to Syria, it has recognized the limitations of military power in the accomplishment of its objectives, especially during times of peace. After establishing the initial framework for his analysis, Karsh provides a chronological examination of the Soviet–Syrian relationship from approximately 1970 until the Gorbachev era. He notes the great concern among the Syrians for the negative implications of "new thinking" in the foreign policy area as it emerged under Gorbachev after 1985.

As Karsh argues, Gorbachev has recognized the legitimate interests of the West in the region and has emphasized the need for a negotiated settlement of the central issues that divide Israel from its Arab neighbors. The overriding objective of Soviet policy in the area from Brezhnev to Gorbachev has been the attainment and preservation of stability. While Gorbachev's approach to the attainment of these objectives has shifted, he has not abandoned the core goal of Soviet policy over the past two decades. This conclusion coincides quite well with the basic argument presented by Freedman.

Oles M. Smolansky's *The USSR and Iraq* is the first major study of Soviet–Iraqi relations to appear in English. Rather than providing a chronological survey of the evolution of relations, Smolansky focuses on specific aspects of that relationship. He does begin with a brief historical overview of relations from 1958 up to 1980 and the outbreak of the Iran–Iraq war. Smolansky argues that relations were characterized by both an overlap of short-term objectives and also by substantial disagreements on issues of major importance. Smolansky then examines several issues that exercise major influence on the relationship. During the first of these, the nationalization of the Iraqi oil industry in the early 1970s, the Soviet Union was strongly supportive, for Soviet and Iraqi interest tended to overlap. The second issue, the Kurdish question, resulted in periodic divergence between the Soviet Union and Iraq. However, by the mid-1970s and the culmination of the governmental suppression of the Kurdish movement, Moscow shifted its support back to the government of Saddam Hussein. The issue of the Iraqi communist party was one that also resulted in a periodic deterioration in Soviet–Iraqi relations. Though the Soviets, on occasion, virtually abandoned their communist allies, by the end of the 1970s when Saddam Hussein executed a substantial number of Iraqi communists, Soviet–Iraqi relations suffered seriously.

Besides issues of Iraqi domestic politics that exercised an influence on the relationship, issues concerning regional problems—Iraq's relations with Arab countries, as well as with Iran—were important. These issues culminated in the Iran–Iraq War of the 1980s, during which the Soviet Union greatly reduced its support for its Arab ally, either asserting a neutral position or on occasion even "tilting" in support of Iran.

The USSR and Iraq makes a significant contribution to our understanding of Soviet relations with an important Arab country. What emerges is a clear picture of a relationship characterized almost as much by divergence and conflict as it was by agreement on key issues of domestic and foreign policy. At no time could Iraq be viewed as a client state of Moscow. Despite the strengths of this book—and they are overriding in any overall assessment—one could wish that greater attention had been given to aspects of Soviet–Iraqi relations during the 1980s not directly associated with the war against Iran. Moreover, one might question the author's decision to exclude any attention to Iraq's role within the Arab–Israeli conflict and also the Soviet role in Iraqi–Syrian relations.

As all three of these well-written books demonstrate, Soviet policy throughout the Middle East has focused on efforts to stabilize conflict and protect Soviet interests. However, despite the infusion of massive resources, the Soviet Union generally was not able to impose its will on client states. In sum, the Soviet ability to affect events througout the region was very limited.

University of Illinois,
Urbana–Champaign ROGER E. KANET

The Political Economy of Third World Intervention: Mines, Money, and U.S. Policy in the Congo Crisis. By David Gibbs. Chicago: University of Chicago Press, 1991. 264p. $29.95.

In a provocative study of U.S. foreign policy in the Congo crisis of 1960–65, David Gibbs provides a convincing reinterpretation of the motivations behind the interventionist policies of the Eisenhower and Kennedy administrations. Gibbs argues that foreign investors helped determine U.S. policy by influencing bureaucratic decision making in each administration. In the Eisenhower administration, key executive branch officials including the secretaries of State and Defense were close to business groups with investments in Katanga Province. According to Gibbs, the economic ties of Eisenhower officials to U.S. investors in Katanga conditioned their support for the attempted Katanga secession of 1960. In contrast, business groups close to the Kennedy administration were divided into pro-Katanga and anti-Katanga factions. The anti-Katanga faction consisted of business groups (e.g., Chase Manhattan Bank) who opposed the Katanga secession due to their economic competition with Belgian companies. These investors sought the establishment of a stable central government in order to facilitate increased U.S. investment in the Congo and to weaken Belgian economic interests concentrated in Katanga Province. Gibbs argues that the divisions within the Kennedy administration between pro-Katanga and anti-Katanga business factions explain the bureaucratic rivalry over whether to support the Katanga secession.

Drawing on a wealth of primary documents, Gibbs challenges many of the traditional claims made by scholars by U.S. foreign policy. He argues that U.S. policy toward the Congo cannot be fully explained in realist or anticommunist terms. Traditional accounts of U.S. intervention in the Congo focus on Patrice Lumumba's communist ties as the primary factor motivating U.S. policy. Gibbs concludes that these accounts are misleading for two important reasons. First, the anticommunist argument fails to explain why the Eisenhower administration supported the Katanga secession, given its destabilizing effects on the Congo and the degree to which it motivated Lumumba to seek aid from the Soviet Union. The Eisenhower administration could have opposed Lumumba without supporting the Katanga secession. Second, there is ample evidence that Lumumba was not a communist and, in fact, sought aid from the United States and the United Nations several weeks before he accepted Soviet assistance. In fact, Lumumba received aid from the right-wing Liberal party of Belgium in addition to aid from the Belgian communist party (p. 99).

These factors lead Gibbs to advance a business conflict model to better explain U.S. policy toward the Congo. The model puts forward two propositions concerning U.S. foreign policy: that business groups are the most influential pressure groups in the foreign policy process and that business groups are divided by diverse sectoral economic interests. This interpretation challenges bureaucratic accounts of U.S. foreign policy which focus on the institutional dynamics of foreign policy decision making. The author provides an additional counterpoint to traditional pluralist and Marxist accounts of U.S. policy. Pluralists underestimate the importance of policymakers' informal business connections while Marxists exaggerate the unity of business groups in the political process.

However, Gibbs is careful to note that the business conflict model is not always the best model for explaining U.S. intervention. He insists that the model is insufficient in explaining the eventual decision of the Kennedy administration to oppose the Katanga secession. Here, the realist or statist model proves useful in accounting for the triumph of antisecessionist business interests close to the Kennedy government. Kennedy officials came to oppose the Katanga secession due to the pervasive fear that it would destabilize the country and lead to increased Soviet and Chinese influence in the region. This Cold War consideration allowed business interests opposed to the secession to win the arguments with their pro-Katanga rivals. In this particular instance, the business conflict and realist models are complementary in their ability to explain U.S. foreign policy toward the Congo.

This is a first-rate example of careful archival research and nuanced scholarship. Gibbs supports his claims with extensive primary documentation, much of it recently declassified. However, the theoretical framework could be enhanced by considering in more detail how the business conflict model might be compatible with other approaches to foreign policy. For example, what does the model have to say about the institutional variables which might influence whether or not one business group triumphs over another in the foreign policy process? Do business groups attain influence solely as a result of their informal connections to policymakers (as Gibbs suggests), or is such influence conditioned by the nature of executive–congressional interaction? Also, to what extent can domestic opposition groups influence U.S. policy by pressuring business and political elites to modify or alter their positions (as during the Vietnam War)? To his credit, Gibbs considers some of these questions in the last chapter of the book, suggesting both cases where the model could be usefully applied and cases where it would be inadequate.

Florida International University RONALD W. COX

The Lessons of History. By Michael Howard. New Haven: Yale University Press, 1991. 217p. $27.50.

In this book distinguished military historian Michael Howard collects a set of articles, essays, and lectures mostly written during the course of the tumultuous 1980s. The perspective in these urbane, thoughtful pieces is one that can at once beguile and frustrate political scientists. On the one hand, Howard is quick to note that historians "find it as difficult as anyone else to distinguish between the significant and the transitory" (p. 2) and urges "humility" in finding historical palliatives to apply to the present (p. 5). On the other hand, he often finds himself uttering generalizations of the most sweeping sort. Since, however, the generalizations emerge after a lifelong study of war and of society and since they are filtered through a mind that is reflective, agile, analytic, and careful, they require consideration and respect.

Howard's chief generalization concerns historical patterns of war, and many are likely to find his conclusions to be wildly optimistic. He expects that war will persist among "undeveloped" societies and suggests that civil war might still occur within both undeveloped and developed ones. Nevertheless, he believes it "quite possible that war in the sense of major, organised armed conflict between highly developed societies may not recur, and that a stable framework for international order will become firmly established" (p. 176). His more fleeting prognoses on economics (not his strong suit) suggest utter gloom: revolutionary socialism, he observes, "is disastrously inadequate for long-term economic development," while its alternative, free-market capitalism, creates "horrifying inequalities and widespread corruption, which in turn inhibit economic growth" (p. 5). Apparently, nothing works.

Howard's intriguing conclusion that major war may have become obsolete derives chiefly from a set of observations about the way people in the developed world have changed their attitudes toward war. At one time, he notes, they were organized into "warrior societies" in which warfare was seen to be "the noblest destiny of mankind." This was changed by industrialization, he argues, which "ultimately produces very unwarlike societies dedicated to material welfare rather than heroic achievement," with the result that "today everyone in developed societies belongs to 'the peace movement', even those who, in the name of stability, are most zealously building up their national armaments" (pp. 167, 175–76).

The main problem for this generalization (as Howard is fully aware) is that industrialization spoke with a forked tongue. If it encouraged some people to abandon the war spirit, it apparently propelled others to fall, if anything, more fully in love with the institution. Some of the finest pieces in the book, in fact, trace the persistence—even the rise—of a militaristic spirit that became wedded to a fierce and expansionistic nationalist impetus as industrialization came to Europe in the nineteenth century. And of course, in the next century industrialized nations fought two of the greatest wars in history. The lessons of history, then, suggest that industrialization can inspire bellicose as much as pacifist tendencies.

Howard never really provides much of an explanation for how or why industrialization must inevitably lead to an antimilitary spirit. The horrors and holocausts that accompanied industrialization he simply attributes to the "growing pains" of industrial societies (p. 1).

For a man so keenly attuned to the force of ideas in history, Howard seems unnecessarily anxious to come up with a mechanistic, necessitarian explanation for attitude change. People in the developed world may have become disillusioned with war as they had become disillusioned with slavery. Because of unpleasant experiences with it and because of the machinations of skilled propagandists, they changed their minds about what they most valued and came increasingly to view war—at least war in the developed world—as immoral, uncivilized, disgusting, futile (particularly economically), and rather ridiculous. It is not at all clear they needed industrialization to come to that view.

University of Rochester JOHN MUELLER

Code of Peace: Ethics and Security in the World of the Warlord States. By Dorothy V. Jones. Chicago: University of Chicago Press, 1991. 208p. $24.95.

In *The Twenty Years' Crisis, 1919–1939* (1949), E. H. Carr argued that idealism and realism play vital interactive roles in the theory and practice of world politics. Scholarship in the postwar era has tended to ignore this interaction, suggesting that we live in a realist world in which ethical principles have little place. *Code of Peace* offers a lucid and persuasive challenge to this position that indirectly recalls Carr's influential study.

Code of Peace describes how a universal set of ethical principles has evolved in the twentieth century. This code expresses an image of an ideal world of peace, justice, and cooperation and serves as a standard against which the behavior of states can be measured and toward which their actions can be directed. Forged by leaders acutely aware of the appeal of short-term national gains, it is the product of their reflections on state relations and their attempts to develop ethical principles and viable institutions to guide these relations. States do not always respect this code, but they recognize it as the essential basis of a just and peaceful world.

Dorothy Jones's argument is presented in six chapters that take the reader from the turn of the century to 1989. The self-interested "world of warlord states" (p. 8) was shocked by World War I into developing institutions to promote peace. Although the most familiar of these, the League of Nations, is best remembered for its failures, her discussion of the much-neglected interwar period notes several successes. This "experimental" period was hampered by conflicting approaches to conflict resolution; an unshakable commitment to state sovereignty; and uncertainty about how to address revolutionary violence, secessionist movements, and human rights violations. But it produced a baseline against which later efforts could be measured; some of its institutions, such as the International Labor Organization, succeeded; and states gained experience in cooperation and problem solving. Unfortunately, its principal strategy for peace, which relied on collective security and public opinion, failed dramatically.

After World War II, elements of the interwar framework for peace were revised and refined. International conferences focused on identifying fundamental principles, as well as developing viable institutions. New states later extended these principles, most notably through the inclusion of an economic conception of social justice. The notion that states existed to serve the needs of people was expressed through conventions on human rights. Thus, over a period of 70 years, nine principles were worked out and repeatedly affirmed that reflect a universal desire for justice and peace. Two auxiliary principles, concerning distributive justice and protection of the environment, have recently received attention and may become part of the code. Jones concludes, "In the short term, the states' efforts may seem to make no difference at all, but in the long perspective of history, this code of international ethics stands as a possible bridge between the world the warlords have made and the world the warlords have dreamed" (p. 166).

Code of Peace was awarded the 1991 Lionel Gelber Prize. It is a model piece of scholarship that includes a clear and rigorously researched argument, several useful appendices (including a list of the 79 conventions and agreements that form the basis of Jones's code), and a bibliographic essay. It will be of special interest to those who study international law, organizations, and ethics but also offers insights into perennial debates over realism and idealism, independence and interdependence, and theory and practice. The text is suitable for teaching purposes and provides a good framework for discussing emerging agreements in areas such as development rights and environmental policies.

The book has only minor weaknesses. Jones's argument that the code of peace transcends cultural differences and is therefore universal is rather cursory. The extent to which the priority, meaning, and commitment given to these principles vary across cultural and other lines requires further attention. The state-centric focus of this study may limit its value in a world increasingly shaped by diverse nonstate actors. The contention that the code of peace is significantly more than the hollow rhetoric of states remains disputable. While it is noteworthy that states have expended great efforts to construct this code, a more cynical mind might emphasize the variety of violence manifest in the postwar era, attributing cooperation and the absence of war in some regions to self-interested cost–benefit calculations. Nonetheless, Jones's central claim that states have gradually created a universal code of ethical principles and a complex of supportive institutional arrangements is well argued. Her study stands as an excellent piece of scholarship that will reward readers throughout the field.

University of British Columbia RICHARD A. MATTHEW

Trappings of Power: Ballistic Missiles in the Third World. By Janne E. Nolan. Washington: Brookings Institution, 1991. 209p. $29.95 cloth, $10.95 paper.

The world watched transfixed as Saddam Hussein lobbed Scud missiles toward Israel and Saudi Arabia during the Gulf War. The military impact of the Iraqi

Scuds was small, but the political and psychological impact of Iraq's ability to throw inaccurate ballistic missiles at her neighbors was great. The issue of Third World ballistic missile proliferation has remained salient. The United States recently threatened to intercept a North Korean freighter suspected of carrying Scud–C missiles to the Middle East. In the wake of these events, Janne Nolan's balanced and informative book is an excellent introduction to the issue of ballistic missile proliferation, an issue that is having a major impact on the global distribution of power.

That Third World countries want ballistic missiles should come as no surprise. Nolan rightly notes, "Given an international system in which military prowess has been a leading determinant of national stature, the efforts of third world nations to acquire the most advanced weapons available in the international market should come as no surprise" (p. 16). Nor should it come as a surprise, she says, that Third World countries have been able to acquire ballistic missile technology. She outlines three strategies that Third World countries have pursued to acquire ballistic missiles.

Some countries have used the establishing of a space program as a launching pad for the development of a ballistic missile infrastructure. India received help in the development of its space industry from the United States, West Germany, France, and the Soviet Union. In pushing the development of space technology, those countries inadvertently pushed forward India's ballistic missile program.

Some countries have been pushed by the United States to develop an indigenous defense industry. The United States adopted this strategy in the early 1970s as part of the Nixon Doctrine, which urged U.S. allies to build their own defense industry to acquire the means to defend themselves. South Korea exemplifies this strategy, as it obtained most of its ballistic missile technology from the United States.

Finally, some countries, such as Iran and Iraq, have relied on external suppliers for the technologies needed for ballistic missiles. Here, the limits of the Military Technology Control Regime (MTCR) of 1987 are apparent. The industrialized nations have been unable to create an effective international regime to stem the flow of ballistic missiles to the Third World.

Those interested in international relations theory might find this book disappointing. Nolan at times alludes to important themes in international relations theory, but they are never fully developed. Though theory development is clearly not the book's goal, there is much data for those interested in the development of international relations theory. Nolan points to both domestic and international factors as explanations for the failure of stemming ballistic missile proliferation. One interesting area for research would be a look at the MTCR from the perspective of the rich literature on regimes: the reasons for the lack of an effective regime should be explored for this case.

In addition, Nolan's chapter on the bureaucratic dimension of U.S. policy toward arms transfers and ballistic missile proliferation is of particular interest for U.S. foreign policy specialists. Nolan aptly notes that the institutional structure responsible for these issues has lagged behind technological changes. Both intellectually and institutionally, U.S. policy toward this emerging issue remains fragmented without adequate coordinating mechanisms. That the bureaucratic apparatus within the American state has not changed despite the increased importance of this issue is consistent with the emerging literature in international relations that has focused on the importance of domestic political structures. Though Nolan does not make explicit note of this literature, her findings provide support for those who have stressed the importance of institutions for explaining foreign policy outcomes.

The author does not discuss another possible explanation for the lack of a coordinated policy toward ballistic missile proliferation: perhaps policymakers have judged ballistic missile proliferation not to be a problem at all. They may feel that the limited range of Third World ballistic missiles does not threaten U.S. interests. Or perhaps policymakers have judged that ballistic missile proliferation will not lead to increased conflict in the Third World. After all, U.S. policymakers have always stressed the deterrent and stabilizing effect of U.S. ballistic missiles and thus may see Third World proliferation through the same lens. Such a perspective is short-sighted, but it may partially explain why the industrialized nations have failed to pay adequate attention to a growing security threat.

Nolan shows that Third World ballistic missiles have had a limited military impact. For example, they are inaccurate and have a shorter range than many of the combat aircraft in the Third World. However, ballistic missiles do offer some advantages with their increased speed and ability to elude air defense. In addition, the lack of accuracy will not last forever (though Nolan does make clear that great technological obstacles make this a problem).

Policymakers must carefully identify the nature of the threat. With the Soviet threat receding, some U.S. policymakers have justified increased funding for the Strategic Defense Initiative on the basis of the Third World ballistic missile threat. However, at this time, China is the only developing nation that can pose a threat to the United States with ballistic missiles. Thus, U.S. policymakers must be careful to respond appropriately to this growing security concern, and must not take steps that would inadvertently exacerbate the threat. For example, the Pentagon has recently announced that it is considering reorienting U.S. nuclear targeting toward Third World countries. Unfortunately, this policy may serve as an impetus to nuclear and ballistic missile proliferation.

The author is careful to note that the threat from the proliferation of ballistic missiles may soon increase. Even inaccurate missiles present a serious problem if coupled with chemical or nuclear warheads. In this sense, Nolan's suggestion for increased attention to this issue and for certain institutional reforms to cope better with this problem are crucial. It is also clear that the United States cannot go at it alone and that multilateral solutions are essential. Nolan's analysis and prescriptions provide a good starting point.

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ERIC MLYN

Law and Morality in Israel's War with the PLO. By William V. O'Brien. New York: Routledge, 1991. 342p. $45.00 cloth, $19.95 paper.

It is an unfortunate but understandable reality that books on the Arab–Israeli conflict are often judged as much by their consonance with the political views of partisans as by their intrinsic intellectual content. This will probably be true with William O'Brien's *Law and Morality in Israel's War with the PLO*, too—all the more because of the difficult and contentious ground it seeks to tread.

Thus, those who tend to emphasize the paramount importance of Israeli security will likely welcome this volume. O'Brien argues that ever since the initiation of armed attacks by the PLO in 1965, Israel has been engaged in a "just war" against an organization whose primary strategy has been one of "terrorist attacks" (pp. 3, 7, 320). Any attempt to ascertain the legality of Israel's action, he holds, must be set in the context not only of the formal content of international law but also a realistic appraisal of both the continuing nature of the conflict and the actual (as opposed to rhetorical) practice of other states in the international system. On this basis, he concludes that the actions of the Israel Defense Forces have been militarily discriminate and proportionate and hence in reasonable conformity with the laws of war (pp. 168–69, 210). The few exceptions are limited aspects of retaliatory policy in Lebanon and the conduct of the 1978 invasion, some Israeli shelling of Beirut and the Sabra–Shatila massacres of 1982, and the behavior of some soldiers since the start of the Intifada in the occupied territories.

By contrast, those who place greater value on Palestinian self-determination will be unconvinced. Their largest objection is likely to be the extent to which O'Brien reduces the conflict to one of PLO terrorism and Israeli response. The former term—ubiquitous throughout the volume—includes a great many acts carried out by groups outside the PLO (including groups the PLO has sought to suppress) and is also stretched to include such things as attacks on military personnel and vehicles in the occupied territories (p. 224). Moreover, by raising the wholly unrealistic specter of the destruction of Israel—and by implicitly or explicitly citing a standard of international practices that includes France's brutal counterinsurgency in Algeria; South Vietnam's treatment of captured Viet Cong; and the human rights practices of Saudi Arabia, Syria, and Iraq—O'Brien is able to defend the many thousands of civilian deaths exacted by Israeli attacks in Lebanon, the policy of beatings adopted to combat the Intifada (p. 258), official torture (p. 254), the closure of Palestinian schools (p. 249), and violations of the 1949 Geneva Conventions relating to prisoner-of-war status (pp. 197–201) and the deportation and collective punishment of civilian populations under belligerent occupation (pp. 259–260). In general, the sorts of charges leveled by Amnesty International, Human Rights Watch, al-Haq, B'Tselem, and the U.S. State Department receive far less attention than Israel's self-justifications. A number of factual errors could be pointed to, including the claim that the PLO initiated military action in 1965. (The action in question was attempted by a group not then part of the PLO.) O'Brien's treatment of Israel's decision to invade Lebanon in 1982 can be seen as seriously flawed, both in its treatment of the *causus belli* and in its failure to adequately address the extent to which that war was directed at installing a compliant government in Beirut. Finally, there is much that he simply does not deal with, ranging from the full activities of Israel's proxy "South Lebanon Army" to the legality of Israeli settlement activity in the occupied territories.

My own views are mixed. O'Brien's analysis is, indeed, clearly framed and thought-provoking. He is certainly correct in underlining the necessity of treating the conflict as a protracted and continuing one wherein individual events cannot be treated in isolation. Yet O'Brien commits precisely this error, focusing on sporadic acts of terrorism at the expense of a real understanding of the Palestinian Nationalist movement, and isolating the morality of Israel's military activities from the morality of its military occupation and opposition to Palestinian self-determination. This problem becomes particularly acute after 1977, with the accession to power in Israel of the hardline nationalist Likud party. Likud policies toward the PLO were clearly driven more by ideology (territorial claims to the West Bank, hence the need to suppress independent Palestinian political organization) than by concerns over the limited threat to Israeli security posed by Palestinian "armed struggle." Indeed, as Avner Yaniv has shown in the Lebanese case (*Dilemmas of Security*, 1987), Israel often used military force to preempt or undermine growing Palestinian moderation that might otherwise have led to a negotiating process and future political settlement involving territorial compromise.

For these reasons, O'Brien's study cannot really be seen as the "comprehensive and objective analysis" that its dustjacket proclaims. However, it does represent another contribution to the continuing legal, moral, and political debate regarding the Palestinian–Israeli conflict.

McGill University REX BRYNEN

On Internal War: American and Soviet Approaches to Third World Clients and Insurgents. By William E. Odom. Durham: Duke University Press, 1992. 271p. $29.95.

This book examines how the United States can best respond to internal wars backed by the former Soviet Union (and others) in key Third World countries and areas. The author, William Odom, selects three countries of concern to the United States—El Salvador, Guatemala, and the Philippines—as well as the Middle East and Southwest Asia, to determine what the United States can and should do in light of the actuality or threat of internal wars that will affect important American interests.

Odom's key contribution is to combine political development theory (with heavy emphasis on Samuel Huntington's work) with knowledge of counterinsurgency warfare (he was director of the National Security Agency and retired as a lieutenant general in the U.S. Army) to guide American policy with regard to internal warfare. He observes that the prospects for democracy in each of the countries he studies are not good. The economic, social, external, and cultural forces are just too weak to support democratic rule over the long term (although attempts at democracy are not precluded). Moreover, in the wake of the Cold War, Odom correctly sees instability rising, as the restraining impact of the superpowers on Third World conflicts diminishes.

Odom's key conclusions are both startling and persuasive. American assistance to governments fighting insurgencies does far more harm than good. Militarily, it saps the ability and incentive of the government to defeat the insurgents on their own. Economically, it stands in the way of the government's establishing effective tax extraction measures. By way of example, Odom contrasts Guatemala's relatively successful efforts at suppressing an insurgency and bringing about economic development with El Salvador's often disastrous results. The key distinction for Odom was that Guatemala had to act with virtually no U.S. assistance, whereas El Salvador relied on massive American support.

Odom also challenges the conventional wisdom by arguing that external forces play a key role in internal wars. He makes the case that outside assistance (from the USSR, Nicaragua, and Cuba) has been crucial for the El Salvadoran and Guatemalan rebels and that outside support (from the USSR and others) has even played a significant role among the Filipino insurgents. He concludes that the most effective way for the United States to defeat internal wars is to strike at external sources of support. Odom acknowledges that this will often be politically impossible (and perhaps unwise, as well); but he makes a convincing case that if the United States is serious about defeating internal wars, it should either go to the source or stay out of the conflict entirely.

Odom concludes with a set of thoughtful policy recommendations. They include that the American government narrow its focus to specific areas where troops are likely to be engaged when planning force development and operations, reduce the size of counterinsurgency forces while improving their quality, develop small countercoup and rescue forces, redefine international law to allow the United States to cope with internal wars, and transfer principal responsibility for paramilitary forces from the CIA (where they do not receive adequate attention) to the Department of Defense.

There are some problems with this otherwise valuable book. Discussions of Soviet strategy in the Third World have a Cold War flavor that has been largely overtaken by events. The book suffers from repetitiveness (the phrase, *colonialism by ventriloquy*, is especially overdone), does not make the case why vital American interests are engaged by internal wars, and treats the Middle Eastern and Southwest Asian countries superficially. The informal and somewhat eclectic style of the book may put off some academics who seek a more rigorous scholarly approach. In addition, Odom does not adequately explain why outside support can be so vital to insurgents and yet so harmful to incumbent governments.

These quibbles aside, this is an important book. As Odom notes, even the collapse of the Soviet Union will not stop internal wars that affect American interests. Leninist precepts on seizing and holding power have not lost their relevance in the wake of the Cold War. Outside powers hostile to the United States (e.g., Cuba, North Korea, and Iran) may continue to play a role in assisting insurgencies. Most important, the notion that the United States can do more by doing less is a refreshing change from much of the burgeoning literature on "low-intensity conflicts" and warrants serious consideration. That this view comes from an individual who has combined a successful career in scholarship with firsthand knowledge of actually dealing with insurgencies adds a special potency to the analysis. Policymakers and scholars will do well to take heed.

Johns Hopkins University STEVEN R. DAVID

Joint Ventures in the People's Republic of China: The Control of Foreign Direct Investment under Socialism. By Margaret M. Pearson. Princeton: Princeton University Press, 1991. 335p. $39.50.

Beautiful Imperialist: China Perceives America, 1972–1990. By David Shambaugh. Princeton: Princeton University Press, 1991. 326p. $35.00.

Since its Open Door Policy of the late 1970s, China's role in the global political economy has concerned scholars from both inside and outside the mainland. In the West, in particular, China's new identity in the world has prompted China scholars to break from the traditional idiographic approach of area studies and utilize more general theories, concepts, and arguments of social science to help us better understand and explain developments in China and their repercussions for the world. Two recent attempts are David Shambaugh's *Beautiful Imperialist* and Margaret Pearson's *Joint Ventures in the People's Republic of China*. Shambaugh examines the impact of (elite) perceptions on (foreign policy) behavior, and Pearson investigates the ability of a socialist state to control the costs of foreign investment while absorbing its benefits. While both are commendable in their attempts to match theory with substance, they remain more grounded in the latter than the former.

David Shambaugh's *Beautiful Imperialist* presents the views, analyses, and prescriptions of China's "America watchers" from 1972 to the present. Three main questions motivate his study: (1) "What is the substance of [the America watchers'] articulate perceptions, what is the variation among them, and how have they evolved over time?" (2) "What are the key explanatory factors (political, diplomatic, intellectual, sociological) that shape the specific perceptions and the images that lie behind them?," and (3) How does perception affect policy? (p. 35). He draws on extensive published writings (both public and restricted) and lectures of China's America watchers, as well as his personal interviews with these analysts.

Shambaugh propounds the thesis that two schools of thought have emerged among China's America watchers: Marxist and non-Marxist. (A third, more traditional and nationalist group appears only in foreign policy analysis. The "hegemonist" school consistently accuses the United States, as well as the Soviet Union, of superpower chauvinism. This approach, says Shambaugh, originates from China's historical experiences during the "century of humiliation" and thus represents an historical, rather than ideological, legacy.) The Marxist group is distinguished by its rigid adherence to ideology, which insists on "deductive analysis": "Marx says X or Lenin says Y, ergo this must be the case in the United States. Thus, the Marxists proceed on this basis to marshal evidence to sustain the a priori theories" (p. 136). The non-Marxist group, however, is less ideologically motivated and more interested in interpreting current events "in a largely atheoretical manner that presents the facts to their readership" (p. 86). Regarding the U.S. economy, for example, Shambaugh adds that the non-Marxists "are not political economists but real economists whose evidence is more empirical and statistical" (p. 136).

By characterizing China's non-Marxist analysts as "atheoretical," "less ideological," "ad hoc," and "descriptive," Shambaugh suggests that they operate outside any normative, theoretical, or ideological boundaries. This presumed neutrality, therefore, indicates that they have no self-serving agenda like the Marxist analysts. Yet Shambaugh fails to notice that these so-called free thinkers accept the same units of analysis, methodological assumptions, and institutional framework of Western neoclassical economics and positivist realism. On economic issues, for example, the non-Marxists focus on interest rates, inflation, tax cuts and money supply, government regulations on business, deficit financing, and federal expenditures. They accept American intellectual conventions (i.e., inflation vs. unemployment, Democrats vs. Republicans) and do not question their validity or even relevance. On foreign policy, China's non-Marxists sound like typical realists, with their emphases on superpower politics, the arms race, mutual deterrence, and U.S. and Soviet interventionism in the Third World.

Divergent intellectual traditions in China warrant attention and identification. But just because China's non-Marxists are beginning to sound like Western technorationalists, they are not necessarily, therefore, "nonideological" and "real," as opposed to the "polemical unreality" of Marxist–Leninists. Shambaugh concludes by declaring that a "perception gap" continues to plague United States–China relations: "Both sides are imbued with severe cases of cognitive dissonance, wishful thinking, stereotypical imagery, misinformation, and misperception" (p. 302). Indeed, this condition goes both ways when scholars of either side hide under the protective belt of "objective rationality" and charge their counterparts with ideologically skewered analyses.

Equally important, this book fails to address the methodological weaknesses of individualistically based, cognitive analysis. For example, Shambaugh presumes that individual input (i.e., China's America watchers) leads to a collective outcome (Chinese foreign policy decisions). Aside from questioning the methodological validity of linearly extrapolating from the individual to the group, another issue arises: What accounts for the content and nature of individual input and its variations over time? Shambaugh himself attributes the rise of non-Marxist analysis in China to increased exposure to the United States and changing institutional demands. But additional questions nag at the reader: Why and how did these structural and institutional changes occur? Why did these changes assume one particular trajectory and not another? Such inquiries expose the liabilities of Shambaugh's premise that "behavior is principally a function of perception" (p. 3). Clearly, perception and behavior interact, but Shambaugh acknowledges only one side of the relationship. Until these ideological and methodological issues are redressed, cognitive studies of this variety provide limited explanations while offering, at the same time, rich sources of textual data and information.

In contrast, Pearson's study of Chinese state control of foreign direct investment demonstrates methodological rigor and avoids ideological self-congratulation. Using China as a case study, she tests the supposition suggested by some *dependencia* and other critics that "socialism in some form would overcome the problems experienced in capitalist developing countries and would provide a substantial increase in host bargaining power" (p. 13). She shows how the Chinese state has tried, through a variety of measures, to "contain" the negative effects of foreign investment (e.g., loss of sovereignty, loss of "Chineseness") while reaping its benefits (e.g., technology transfer, foreign currency, demonstration effects). Pearson accomplishes this by focusing on host country controls at the national, regional, and enterprise levels. Given its rich data base and meticulous research, this study offers a firstrate history of foreign investment in China from 1978 to 1988.

Pearson concludes that a socialist state does not necessarily exert greater control over its foreign direct investment sector; indeed, China's record is mixed. While the Chinese state was successful in maintaining some control in foreign direct investment sectors (e.g., enterprise welfare provisions, externally imposed economic regulations), it eventually ceded a certain level of authority to foreign management (e.g., obviating political study sessions and political secretaries in enterprises): "At the most basic level, some of the specific tools the government intended to use to control the negative effects of foreign investment were simply ineffective in the post-Mao environment" (p. 203). Equally important, China's liberalization process effected a change in the relations between the state and transnational corporations as Chinese economic reformers increasingly conceded that the overall economic benefits of foreign investment outweighed its potential social costs.

Pearson, however, remains curiously silent on a potentially significant contribution suggested by her research: If the nature of the host state (i.e., capitalist, socialist) matters little in its long-term ability to control or contain transnational corporations, then what does matter (if anything)? Pearson does not address this logical implication of her findings. Instead, she discusses the generalizability to other socialist states of her findings on China. But this discussion seems moot, given Pearson's own conclusions. If a continental socialist state as ideologically bound and economically sufficient as China's cannot resist the seduction of foreign capital, technology, and know-how, why should smaller, less well endowed socialist states like Vietnam and Tanzania be able to fare any better? Not surprisingly, Pearson remains pessimistic: "Any expectations that socialism will be adequate to allow these countries to control substantially the terms of investment while absorbing satisfactory amounts of such investment are unlikely to be met" (p. 223).

Furthermore, I disagree with some of Pearson's theoretical assumptions. She accepts too easily the double artificial divide between domestic politics and international economics. The ability of a host state to control foreign investment, Pearson writes, reflects "its domestic political and economic systems, on the one hand, and the international economy, on the other" (p. 9). Indeed, Pearson continues with this line of analysis to speculate on China's future foreign investment environment. "It is certainly possible," she states, "that problems in areas not specifically related to the 'open' policy—inflation, the social upheaval caused by modernization, or growing intolerance over 'bourgeois liberalization'—could spill over into the realm of foreign economic policy and provide an impetus to halt foreign investment" (p. 216). But where do these "domestic problems" come from? Why would an otherwise autarchic, totalitarian state like China's experience inflation, modernization problems, and fears over "bourgeois liberalization" were it not for linkages with the world economy? Such insistence on domestic–foreign and political–economic separations only muddles our understanding of the nature of global interaction.

These criticisms aside, though, both Shambaugh's and Pearson's works represent the "future wave" of China studies. They are to be lauded for striving to match theory with ethnography for a case study as perplexing and fascinating as China's.

Syracuse University L. H. M. Ling

Righteous Realists: Political Realism, Responsible Power, and American Culture in the Nuclear Age. By Joel H. Rosenthal. Baton Rouge: Louisiana State University Press, 1991. 191p. $24.95.

Realism is being put to the test by sweeping transformations in the international society. Traditionalists are looking backward, reinterpreting historical approaches for guidance in a sea of change. Postrealists are looking forward, applying new theories culled from continental philosophy to understand the challenges of late modernity. And neorealists are waiting for the data to come in.

Joel Rosenthal, editor of the journal *Ethics and International Affairs,* is clearly in the first camp. In a very incisive manner he sets out to rescue realism from its critics, as well as from some misbegotten disciples. He does so by going back to its origins, not in the sense of some pristine past of pure realism but of a formative moment in international politics when a public debate among a cosmopolitan group of thinkers and leaders, including George Kennan, Hans Morgenthau, Reinhold Niebuhr, Dean Acheson, and Walter Lippmann, gave rise to an uniquely American brand of political realism. Trying to make sense and a new order out of the postwar flux, these prototypical realists tested the legacy of the U.S. foreign policy (moralistic and legalistic, isolationist and exceptionalist) against the European tradition of realpolitik (where national interest outweighed, but did not ignore, moral considerations). The result, according to Rosenthal, was the advocacy of a hybrid American realism that was philosophical yet practical, prudent but not passive, critical *and* consensual. In short, a "righteous realism" to lead the nation and to instruct the public in the new responsibilities and obligations of a superpower.

In structure and intent the book is a concise review of the lives and thoughts of these seminal American realists. Indeed, the strength of the book lies in its ability to strike a balance between historical context and theoretical content, allowing the author to elucidate the key ideas of realism, demarcate the limits of its influence on American foreign policy, and make the case for its renewal—all in less than two hundred highly readable pages. In the process, Rosenthal does the field of international relations a service by envisioning a more complex realism. The straw man so often constructed by realism's critics is dwarfed by Rosenthal's comparative analysis of how these self-conscious realists used philosophical, historical, and moral reasoning to deal with the pressing issues of the day. There was nothing neat or parsimonious about their theories: the constituent elements of protorealism (the inevitability of tragedy, the play of paradox, the intervention of contingency) made for a highly normative, yet pragmatic, system of thought. This made, realism, the author argues, rather than any competing worldview, the best vehicle for understanding the moral responsibilities and political ambiguities that came with America's new power.

These individual *Ur*-realists unquestionably left their mark on American foreign policy, yet their ideas as a school of thought never really took root in the halls of power, the public realm, or the universities. Rosenthal goes beyond a theoretical and historical approach to explain the limits of their influence by detailing the tension between their professional responsibilities and personal beliefs (and the not always successful attempt to reconcile them). In their pursuit of a public philosophy of realism, they were able to transcend their primary identities as academician (Morgenthau), theologian (Niebuhr), statesman (Acheson), or journalist (Lippmann). They did so without pandering to the public; and although all held at one time or another high positions in the establishment, they eschewed elitist prerogatives. Rosenthal never really makes it clear whether the status was one of choice or circumstance; but the righteous realists were most effective (and perhaps most comfortable) as outsiders, or in Rosenthal's words, "cultural critics."

These realists did run against an American tradition in that they were machiavellian to the extent that they openly recognized the paramountcy of power in both domestic and international affairs. They were not, however, willing to subordinate their principles to the pressing demands of the Prince or to the passing desires of the public. Perhaps they could best be described as "Odyssean" realists: early in their careers they had heard and survived the siren calls of power, wandered and been tested in the wilderness, and then returned to warn the public of grave dangers, of "technocratic illusions" (Niebuhr), "a new feudalism" (Morgenthau), and "the sophisticated mathematics of destruction in which we have been entangled" (Kennan) (pp. 118, 135, 160). Most of them challenged conventional wisdom with nuanced positions of opposition at critical moments in American politics: Niebuhr criticized area bombing in World War II, Lippmann took on McCarthyism in the 1950s, Morgenthau early on came out against the Vietnam War, and Kennan opposed the Reagan arms buildup in the 1980s.

The author puts great stock in historical exposition, intellectual tension, and moral struggle to explain the efforts of the early realists to overcome the isolationism and exceptionalism of American foreign policy. Yet, by leaving out the story of other influences and challenges, the book serves in a peculiar way to reinforce American parochialism. For instance, there is no mention of the important cross-fertilization of American with British realism. What of the claim made by E. H. Carr, the British father of modern realism, that his greatest intellectual debt was to Niebuhr? Or Morgenthau's alignment with Herbert Butterfield and Martin Wight against the "abstract rationalism" of American international relations theory. Can one ignore the role of the Rockefeller-funded Committees on the Theory of International Politics set up in the late 1950s on both sides of the ocean for the study of international theory?

In the end, Rosenthal's account of the fathers of American realism rings true but incomplete. A new appreciation of how these men profoundly influenced American politics is gained; but one is left wondering how these giants of critical and moral reasoning were so easily conquered by the Lilliputians: the game-theorists who made MAD make sense, the systems analysts who rationalized the Vietnam War, the vulgar realists who justified the second Cold War. For that purpose, a sociology of institutional politics is needed, as well as a rhetorical study of how the language and the power of the early American realists, initially steeped in the theologicoethical lexicon of tragedy and providence, justice and order, was scrubbed clean and neutralized by a nascent social science seeking a value-free discourse, thereby handing the public philosophy of international relations back to the ideologues.

University of Massachusetts, Amherst JAMES DER DERIAN

Images and Arms Control: Perceptions of the Soviet Union in the Reagan Administration. By Keith L. Shimko. Ann Arbor: University of Michigan Press, 1992. 288p. $37.50.

Images and Arms Control, by Keith L. Shimko, is a pleasure to read. The book uses, and contributes to, a developing political–psychological framework increasingly referred to as "image theory." It examines important theoretical issues, raises provocative questions that should fuel debate in this theoretical orientation, and applies the framework in an explanation of a series of arms control issues during the Reagan administration.

The theoretical portion of the book should provoke questions and debates among readers interested in political psychology. It begins with a discussion of the concept of images in political psychology and explores the enemy image in detail. The author expands upon a simple one-dimensional assessment of this construct, which is an important task in the refinement of the framework. Shimko argues that subtypes of the enemy image can be developed by fleshing out the attributes of the image further. He suggests, for example, that discriminations among types of enemies are made in perceived distinctions between "dispositional" and "situational" motivational forces. Perceived enemy capability attributes are amplified by adding nuclear and conventional weaponry attributes to the superiority, equality, inferiority perceptions commonly associated with the enemy image.

This effort raises a host of interesting questions concerning the general images concept. One set of questions concerns the problem of conceptualizing an image in a form that is distinct from the actual "fit" of a particular state in the image. One can ask first whether the proposed additional perceived dispositional, situational, and capability characteristics are at a sufficiently high level of abstraction to be useful and cognitively efficient components of an image. If not, they are not likely to be elements of an enemy image at the abstract conceptual level. Instead, these attributes may be ad hoc operationalizations or potential operationalizing categories used by policymakers on a case-by-case basis and therefore crucial elements in furthering the analyst's ability to describe the policymaker's full picture of a state once it is categorized into an image. While Shimko suggests that these are elements of image subtypes, he uses them as indicators for delineating differences in perceptions of the USSR within the Reagan administration.

A second important question raised in this theoretical portion of this volume concerns the nagging question of whether it is theoretically wise to think of differences among policymakers with similar images in terms of subtypes of images or positions upon an image continuum. Shimko prefers the former, but psychological findings should be important considerations in a larger debate on this issue.

The empirical portion of this book should be of interest to all readers, whether they are attracted to the theoretical orientation or not and regardless of differences in methodological demands. It presents a careful, well-supported assessment of the variations in Reagan administration views of Soviet motivations, strategy, decision style, and negotiating behavior. Shimko uses content analysis to provide empirical support for his evaluation of five central figures in the administration: Caspar Weinberger, Richard Perle, George Shultz, Richard Burt, and (of course) Ronald Reagan. The content analysis technique and problems in the methodology are clearly addressed, and the author presents his data in a readable form.

Shimko finds important similarities in images of the Soviets among Weinberger and Perle, on one hand, and Shultz and Burt, on the other. They were shown to differ in terms of both the content and quality of their enemy images of the Soviets, with Shultz and Burt often appearing not only less hard-line but also less certain in their assessments of Soviet motivation and intentions. Reagan is found to contain elements of each camp's image, but is ultimately closer to Weinberger and Perle. The policymakers' use of these enemy images is compared in terms of content, cognitive style, interest, knowledge, and other patterns. Most interesting is Shimko's ability to specify which elements of the images were different. The impact of those differences is then shown in an analysis of the INF, SALT II and START, and SDI debates.

The author's discussion of the impact of images on policy preferences is particularly useful in addressing the problem of how precise an explanation of policy preferences and tactical choice one should expect from political psychology. Shimko shows that the concept can be used easily in explaining general policy preferences and disputes. He also does an admirable job of connecting small variations in the enemy image of the USSR with specific differences among policy advocates concerning not only arms control in principle but their positions on classes of weaponry. He does not make the mistake of demanding that a perceptual analysis explain the exact number of weapons selected, but he does push the concept as far as it will go.

Finally, this book explores the limits of image-based analysis. The author honestly notes and accepts positions that cannot be accounted for with images and suggests a variety of additional explanatory factors ranging from personality characteristics to national self-image. In the process, he may give Ronald Reagan credit for more cognitive wherewithal than he is due; but that too is a mark of the author's effort not to overstep the boundaries of what he can claim for his analysis.

Washington State University MARTHA COTTAM

The Diffusion of War: A Study of Opportunity and Willingness. By Randolph M. Siverson and Harvey Starr. Ann Arbor: University of Michigan Press, 1991. 141p. $29.95.

The question of why most interstate wars remain small and localized while a few become large and, more rarely, global is the central concern of this volume. More specifically, the authors examine the degree to which geographical contiguity and alliance commitments affect the choices of states to join or not join an ongoing war. They also consider whether these factors have any impact on the timing of war-joining decisions. In a nutshell, the reported empirical findings support the following conclusions. States that are contiguous with, or allied to, states already at war are significantly more likely to join the war than those that are not. Moreover, among joiners, states with closer geographical and alliance ties to warring states are likely to join wars sooner than those with looser ties. These findings are based on an examination of the Singer–Small war data for the period 1816–1965. Geographical contiguity is measured in terms of three categories: land border, cross-water border (separated by 200 miles or less of water), and colonial border (which actually includes other types of dependencies such as occupied areas). The alliance data are essentially the Correlates of War set, and the authors employ the familiar three-part distinction (defense pacts, neutrality/nonaggression treaties, and ententes) found in these data in their analysis. With both measures, the relative strength of effect expected on war-joining accords with the order just given; for example, states that have a land border with a warring state are more likely to join an ongoing war than those with a cross-water border. In general, the empirical results do support these expectations.

If the authors did little more than report their empirical results, one might question the justification for the book when one or two journal articles could have conveyed the same information faster and cheaper. (Indeed, some of the results have appeared in articles.) The answer to this lies in the broader purpose of the authors, that is, to show by example how the scientific study of war should be conducted.

Many of those engaged in the scientific study of war have argued for some time that wars should not be viewed as events but, rather, as processes; that is, wars do not simply start and, after a period of time, stop but evolve through time in response to factors that continuously promote or constrain their size, magnitude, and duration. Although such a process orientation may seem obviously appropriate to the study of war, the vast majority of empirical studies of war done in the past have not been in accordance with it. For example, the many time series studies that have used nation-months of war or battle deaths in war as a dependent variable implicitly assume that the factors that account for the occurrence of war are the same as those which account for its evolution. Ignored in these studies is the strong likelihood that new, nonlinear forces are set in motion after the beginning of a war that determine its ultimate character.

According to this process conception of war, there is not *a* war puzzle but, rather, a series of connected war puzzles that require solutions in order to develop integrated, cumulative knowledge about war. A major virtue of *The Diffusion of War* is that the authors embrace this process conception of war and focus on one of the more important war puzzles, namely, Why do most wars remain small, while a few become very large?

Another desirable characteristic of the book is that the authors explore the opportunity and willingness framework more thoroughly than any previous study. According to this conceptual framework, in order for a war to occur, at least two states must have the *opportunity* and *willingness* to engage in combat with one another. Thus, it is useful to categorize the factors that affect the likelihood and character of war in terms of what impact they have upon perceived opportunity and willingness. The authors elaborate this framework and spell out some of its implications. A major contribution of this framework is that it reminds us that a war requires at least two decisions to fight, one to attack and a second to resist. This may seem obvious; but it contradicts the standard implementation of expected utility theory, since, based on objective conditions, the more desirable a war decision is for an attacker, the less desirable it is for a defender to resist. In short, we must explain not only why some states attack (or threaten to attack) other states but also why those other states resist these attacks or threats.

As promising as this conceptual framework is, it does contain some problems. It rests fundamentally upon an ends–means distinction that is hard to maintain even in the abstract. Opportunity engenders willingness and willingness creates opportunities, and the two may be so intertwined as to make a distinction between them impossible. This problem becomes much worse when we turn to the operational side. For example, does a common border affect the opportunity for war or the willingness to war or both? Since the correct answer to this question is most probably *both* and, lacking direct measures of opportunity and willingness, we have no empirical basis to apportion the effect of common border, we are left with an arbitrary assumption about the nature of its effect. This problem is not restricted to geographical factors, and it raises the question of whether opportunity and willingness can be empirically separated in a meaningful way.

The authors should also be credited with directing our attention to the interdependence of events with their focus on the diffusion of war. Almost all standard statistical inference procedures are based on the assumption of independence; and the authors rightly point out that much social behavior (e.g., war joining) violates this assumption, sometimes dramatically. The authors embrace a diffusion–contagion model but (to my disappointment) do not develop, estimate, and test probability models that incorporate interdependence.

A final important contribution of the book is its reinforcement of the "rediscovery" of geography. War studies (and quantitative international politics in general) have neglected the spatial factor in world affairs. Too often, this factor has been considered trivial, uninteresting, or constant (all of which it is not) in previous analyses; and the authors deserve praise for demonstrating its importance.

In contrast to these good points, the book does have some notable deficiencies. The empirical findings are thin in comparison to the expectations raised by the title and theoretical sections of the book. Many factors affect the decision to join a war, and the authors' focus on only two of these is regrettable. Their focus is further restricted by the fact that they are only concerned with whether and when states join wars and not with which side they join. No explicit models are derived or tested, and the methods used are not uniformly state-of-the-art. Finally, although the authors rely on the Correlates of War data, they take exception to a number of Singer–Small coding decisions. These are sufficiently numerous as to warrant a complete listing of the war data used.

In spite of these deficiencies, the book makes an important contribution to the study of war, largely because it points in the direction we need to go if we are to produce scientific, cumulative knowledge about the causes of war and conditions of peace.

State University of New York, Binghamton STUART A. BREMER

Myths of Empire: Domestic Politics and International Ambition. By Jack Snyder. Ithaca: Cornell University Press, 1991. 330p. $35.00.

It is often noted that our theories of the domestic sources of international conflict and security policy have never been as fully developed as systemic, bureaucratic, or individual-level theories. With the publication of Jack Snyder's *Myths of Empire,* this is no longer true. Snyder poses the question of how to explain the numerous cases of overexpansion in the industrial age. He constructs a modified rational choice explanation based on the parochial interests of key domestic and bureaucratic groups, the strategic myths they construct to rationalize their expansionist policies, the processes of coalition formation and logrolling among these groups, and the domestic structures and patterns of industrialization under which these dynamics are most likely to arise.

Earlier theories of imperialism or overexpansion focused on the interests of self-serving monopoly capitalists (Hobson and Lenin) or atavistic military–feudal elites (Schumpeter). These concentrated interests reaped the benefits of overexpansion, while the costs were diffused throughout society in the form of taxation. Snyder, building on theories of collective action and voting theory, argues that groups concentrated enough to benefit from overexpansion are too narrow to control state policy, whereas those broad enough to control policy lack the motivation for overexpansion because their interests are too diffuse to reap net benefits. Snyder argues that key internal groups—each with parochial interests in limited but different forms of imperial expansion, military buildups, or economic autarky—create logrolled coalitions that secure power at the cost of a more expansionist foreign policy than is desired by any single group, and a policy that cannot be supported by available resources.

But logrolled interests do not alone explain overexpansion or the stability of the ruling coalition behind it. Key elites justify their power and policies through strategic myths which suggest that security is possible only through external expansion. Snyder rejects the hypothesis that strategic ideas are derived from intellectually formative lessons from history and argues, instead, that they are constructed by actors to rationalize their preferred policies. But these elites often internalize their own rhetoric or become politically entrapped in it, which inhibits strategic learning and disengagement from increasingly costly expansionist policies.

These dynamics are most likely to arise in cartelized political systems dominated by distinct concentrated interests. Overexpansion is less likely and less extreme when it occurs in democratic systems (where interests are diffuse and where the absence of information monopolies works against strategic mythmaking) or in unitary oligarchies (which have encompassing interests in avoiding the costs of overexpansion). Overexpansion is less predictable in dictatorial systems in which there is no check on the preferences of the leader. Domestic structure is a function of the timing of industrialization, which correlates closely, Snyder argues, with the concentration of power in society and the concentration of group interests. Early industrialization (Britain and the United States) is associated with diffuse elite interests and mass democracy, late industrialization (Germany and Japan) with concentrated interests and a cartelized system, and "late late industrialization" (the Soviet Union) with a centralized system ruled by a unified elite with encompassing interests.

Snyder tests the theory against competing power-based realist and cognitive learning hypotheses for five important cases of overexpansion: Germany from Bismarck to Hitler, interwar Japan, Victorian Britain, and the Soviet Union and the United States during the Cold War. The research design is carefully constructed to allow cross-sectional comparisons across countries, longitudinal comparisons within each country over time, and comparisons of group and individual preferences and strategic mythmaking within a country at a given point in time. The individual case studies are strongly driven by the theory, thoroughly informed by the secondary historical literature, and for the most part quite persuasive.

Snyder's findings support his theory. The most cartelized political systems were the most overexpansionist, whereas democracies and unitary oligarchies were considerably less so. In addition, Germany and Japan were least expansionist during those periods when they were least cartelized, while democratic and unitary states were the most expansionist when concentrated interests were relatively strong. Snyder shows that strategic beliefs were not learned from history but, instead, constructed to rationalize domestic interests and that realist theory cannot account for observed behavior. He concedes, however, that external threats and opportunities influence internal coalition politics and suggests that a more complete theory needs to integrate power, interests, coalitions, and ideology at both the international and domestic levels.

Myths of Empire is the best-developed theory to date of the domestic sources of international conflict and security policy. The fact that it is confined to great power overexpansion in the industrial era raises questions, however, regarding the extent to which the theory can be generalized. Snyder does not pretend to explain overexpansion in the preindustrial era, and he also suggests that the coalition politics model probably does not fit small powers. Snyder's care in not overstating his case should be applauded, though the modification of the theory to apply to the different class, sectoral, and bureaucratic structures of preindustrial states and also of contemporary small states would be a fruitful area for future research.

Although not all overexpansion leads to war and not all wars grow out of overexpansion, Snyder's coalition politics theory is an important contribution to our understanding of the causes of war, particularly the underlying causes. But elite cost–benefit calculations for war may be different from those for expansion, in part because external threats may be more pressing and elites may not be able to pass the political and economic costs of war onto others or defer them to an uncertain future. Although Snyder's concept of self-delusion and political entrapment by earlier strategic rhetoric is undoubtedly important here, decision-making variables and strategic bargaining with the adversary also needs to be included.

The generalizability of the theory may also be limited by its focus on overexpansion as the dependent variable. Although the German and Japanese cases may be rather obvious ones, the concept of *over*expansion is difficult to operationalize, particularly given uncertainties regarding the intentions of others and the consequences of one's own action. Moreover, realist explanations emphasizing state cost–benefit calculations may be more difficult to dismiss for less extreme cases of expansion, though Snyder's novel interpretation of Britain's limited expansion in the Crimean War case demonstrates the potential applicability of his theory to such cases.

But these are just quibbles. *Myths of Empire* is one of the most important books on international conflict to come out in several years. Snyder has constructed a reasonably parsimonious model of coalition politics supplemented, where necessary, by key variables from other levels. In so doing, he has taken a major step toward ending the theoretical impoverishment of the study of the domestic sources of international conflict.

Rutgers University JACK LEVY

Moscow, Germany, and the West from Khrushchev to Gorbachev. By Michael J. Sodaro. Ithaca: Cornell University Press, 1991. 423p. $49.95 cloth, $15.95 paper.

American international relations scholars and specialists on Soviet foreign policy tend to view events in the postwar period largely in terms of U.S.–Soviet relations. While that presumption is often valid, it can sometimes obscure important sources of Soviet behavior by treating it as a derivative of its U.S. policy. This is especially true in the case of Europe, where Soviet fears and hopes with respect to important countries such as Germany have their own logic. By taking the relative importance of the United States in Soviet–German relations as a variable, rather than as a given, Sodaro shows that the leadership and the secondary elite (academics, journalists, etc.) in the post-Stalin era have been divided on a very basic issue in Soviet policy toward Germany: Does the Federal Republic of Germany have any independence in charting its foreign policy course, or is it subordinate to American policy and influence? He argues (and demonstrates quite convincingly) that four widely ranging views on West Germany's role in East–West relations—maximalist, Europeanist, Atlanticist, and Americanist—existed throughout the period in the Soviet and East German leaderships and that the basic prevalence of Atlanticist views created a flexibility in Soviet policy that both made détente possible and limited its cooperative potential. The book also examines how the constraints of the Soviet–East German alliance played a role in the timing and extent of cooperative initiatives due to substantial differences in East German and Soviet interests and viewpoints.

Sodaro's primary concern is to demonstrate that Soviet and East German policy toward West Germany cannot be explained in terms of unitary actor approaches but must be evaluated in terms of domestic political structures and the varying policies and viewpoints within the leaderships. In chapter 1 he sets out four types of decision-making regimes (directive, conservative, centrist, and reformist) and four primary aspects of Soviet foreign policy activity (the scope of détente, degree of responsiveness, willingness to compromise with foreign governments, and openness to internal debate). Because of the internal dynamics of the different types of decision-making regimes, they tend to produce different forms of foreign policy activity. Ulbricht's directive regime assessed détente in very narrow terms, was unresponsive to changes or other states, was unwilling to compromise, and restricted debate. Gorbachev's reformist regime has exhibited the opposite tendencies on all these aspects, while Brezhnev's conservative regime, Khrushchev's centrist–reformist regime, and Honecker's centrist–conservative regimes occupied the middle ranges. Thus, Sodaro claims, variation in foreign policy between leaderships and within leaderships (early vs. late Brezhnev) can be explained only by the imperatives and course of domestic politics.

As such, this book is another important contribution to recent work dissatisfied with relegating Soviet and East European communist decision making to the "black box" simply because of the difficulty in obtaining hard data on politics and foreign policies. Most of the book explores the disagreements expressed by the primary and secondary leaderships and shows that at many points both the Soviet and East German leaderships entertained various policy options, with results due to both relatively constant national interests (primarily the need for West German aid, trade, and technology) and internal political images. For example, Sodaro shows that were it not for Khrushchev's fall in 1964, Soviet–West German détente would have taken an earlier and more comprehensive form, with less of the constraints of military competition that bedeviled policies in the late 1970s. Similarly, it is impossible to imagine a persistent Brezhnev leadership moving to a Gorbachev form of German relations, given their divergent domestic priorities and coalitions.

But this does not make for a *theory* of the role of domestic politics in Soviet and East German policies toward West Germany. It takes the different leaderships as basically given and demonstrates that they have different priorities that affect their foreign policies. Thus, it is not really a theory of domestic politics but of regime types. Evidence that different leaderships or regimes have different priorities and policies does not demonstrate the impact of domestic politics. Indeed, it is consistent with unitary actor models of foreign policy in which the preferences and strategies of the unitary actor change at a few points in time. The author does not use his evidence of debate on policy toward West Germany to test a theory of how disagreements within the five leaderships affected policy. Such a theory would have to show how the institutional structure or nature of power in a political system was likely to produce particular policies. Harry Gelman's *The Brezhnev Politburo and the Decline of Détente* (1984) is an example.

Indeed, the author at times falls victim to the unitary actor model himself in constructing explanations for puzzling policies. The decision to deploy the SS-20s in the late 1970s, when the Brezhnev leadership had committed itself to a strategy based upon the advantages of economic and political détente, seems irrational, given the likely effect on West Germany of an enhanced Soviet nuclear threat aimed directly at it. Rather than exploring how the contradictory imperatives of a diverse coalitional leadership might contribute to self-defeating policies, the author attributes the SS-20 decision to military modernization, the desire to promote U.S.–West German conflict and gain further political advantages from Soviet military power, and a calculated decision to keep the level of military confrontation in Europe high in order to prevent any warming in West German–East European relations (pp. 266–69). This analysis is plausible and may be correct; but it presents a consistent and largely unified leadership and explains Soviet policy by seeking to make the leadership's reasoning coherent.

In the end, these problems are due to the enormous task of sustaining a theoretical framework while documenting and analyzing a complex area of foreign policy in two closed societies for a period of nearly 20 years. As a result, the theoretical issues too often become submerged in the empirical chapters that focus on Soviet and East German official policies and elite debates. The research does sustain the author's primary contention. To understand what these two countries did outside their borders in Europe, one must understand what went on within the borders.

Harvard University CELESTE A. WALLANDER

Encountering the Dominant Player: U.S. Extended Deterrence Strategy in the Asia–Pacific. By William T. Tow. New York: Columbia University Press, 1991. 540p. $55.00.

In a well-written and well-researched book, the author presents an elaborate argument that the United States has too much projected NATO strategic concepts to the Asia–Pacific region. He concedes that South Korea may have been as geographically vulnerable to a Moscow-directed tank attack over the decades as West Germany but notes that the rest of the region has not been beset by threats requiring counterthreats of U.S. nuclear escalation. In light of this, according to Tow, the region has too often simply been used as an auxiliary to concerns about NATO (as with various versions of the "maritime strategy" and "horizontal escalation" and the playing of the "China card").

The author's argument might have been made more tightly, and presented more rapidly and effectively, in a journal article; his overarching logical structure is repeated quite often through the book and may thus inferentially be assigned somewhat more significance for U.S. policy planning than is warranted. Above all, the book (like almost all the books now on the shelf) is overtaken by events—by the collapse of the Soviet Union and by the U.S. "dominant player" role in liberating Kuwait and by the burgeoning fears of nuclear weapons proliferation to North Korea alongside the rapid evolution of the entire Korean situation.

Tow's analysis is basically that *other* security concerns, *or* economic concerns that do not translate into military security problems, have been more important than the deterrence of Soviet attacks as we have tried to optimize U.S. policy for the Pacific rim. The book draws in a great deal of detail and analysis on this region over the entire Cold War period and handles it in a very balanced and persuasive way. Tow also nicely sorts out a fair amount of what we think of as theory on international relations and American foreign policy. Nonetheless, it might be that the U.S. government has already long perceived that, except for Korea, it had no great need for extended nuclear deterrence in the Pacific. In that case, the book may at times be attacking a straw man.

University of Maryland GEORGE H. QUESTER

Securing Europe. By Richard H. Ullman. Princeton: Princeton University Press, 1991. 183p. $19.95.

Richard H. Ullman has boldly ventured a book on the future of European international politics, with the general thesis that, *pace* John Mearsheimer, we will not miss the Cold War. In his admirably lucid and brief book, Ullman argues that "the era Europe has entered will be qualitatively different from those it has known before" (p. 139). Although the international system remains anarchic, among the developed countries the causes of war have become muted and the incentives for peace have increased. Arrangements are within grasp that would render wars growing out of either aggression or arms races and the security dilemma extremely unlikely. Furthermore, the end of bipolarity and the Soviet threat mean that peace is divisible. Conflict in the Third World, Eastern Europe, and within the Soviet Union need not disturb the peace among the OECD countries. With Germany unified, all the countries of Western Europe are now satisfied with the status quo. The revolutionary changes within the Soviet Union and that country's abandonment of ideologically driven need for satellites to the East and fear of the West have rendered that country, too, little desirous for any change that might involve violence. Even without the unifying fear of the Soviet Union, the countries of Western Europe have nothing to fight about. Furthermore, existing institutions (NATO, the European Community) not only provide a framework for mutual economic and political benefit but also permit each state to see that the others are not about to threaten it. The fact that these countries are all liberal democracies contributes mightily to their good relations. The only remote danger Ullman sees is the possibility that other forms of government would appear, especially in Germany, as the result of a severe international depression (pp. 35, 41, 105).

The prospects for the Soviet Union and Eastern Europe are not so rosy—though even here Ullman is not as pessimistic as many other analysts. The ideas and structures of the Conference on Security and Cooperation in Europe and the West European Union could be used to construct a European Security Organization (ESO) that would include the United States and Canada. The United States would have a significant role in Europe, albeit not as unnaturally large a one as it played during the Cold War. Inspection, both formal and informal, could produce confidence and security. Promises of collective security would be credible, in part because there is little danger that a powerful aggressor will arise. One cannot, however, exclude the possibility of "an outbreak of warfare between Hungary and Romania or Greece and Turkey, or between two Yugoslav republics," in which case the ESO might find armed intervention excessively costly and settle for "walling off" the conflict instead (p. 68).

Because "only democracies share a commitment . . . to the rule of law and to the peaceful settlement of disputes," countries with other forms of government would be excluded from the ESO (p. 76). But (as Ullman notes but might have discussed at greater length) excluding the Soviet Union or successor republics if it or they are undemocratic would create problems (p. 77). Together the developed countries could try to limit arms proliferation in, and terrorism from, the Third World and should maintain sufficient military force for some military interventions; but it would be neither necessary nor desirable for the ESO to try to police the Third World. Conflicts there, however unpleasant for the people concerned, are not likely to "spill over to engulf the advanced, industrialized states" (p. 135).

I find this optimism quite believable, but Ullman could have devoted more attention to things that might go wrong. First, an international depression could undermine democracies and create conflict. The role of Japan (excluded from this book) would need to be considered. Second, Ullman may be too sanguine about prospects for democracy in Eastern Europe and the Soviet Union. Third, keeping conflicts among these countries from spreading to Western Europe may be more difficult than Ullman expects, especially if Germany develops strong interests there. Indeed, the economic investment that everyone calls for, while perhaps reducing the chance of violence and war, would render more difficult a "walling off" strategy in the event its pacifying influence fails. Finally, the security dilemma may be troublesome in the absence of the Cold War, especially if each country begins to worry about becoming the target of pressure from a coalition of neighbors.

As Yogi Berra said, "Prediction is difficult, especially about the future." Time will tell whether Ullman's optimism is warranted. But he has made a strong case for it.

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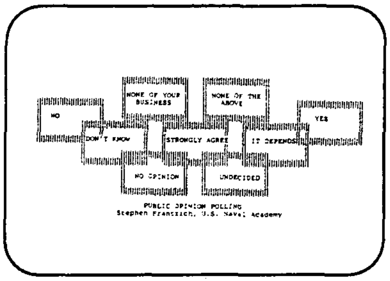

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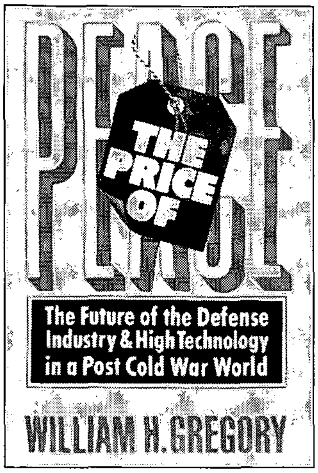
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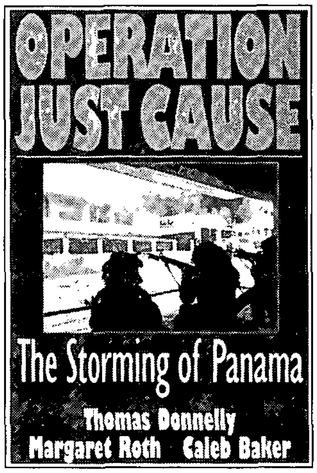

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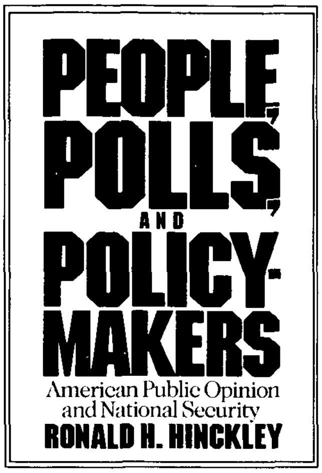
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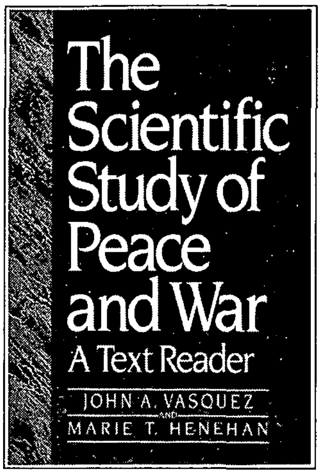

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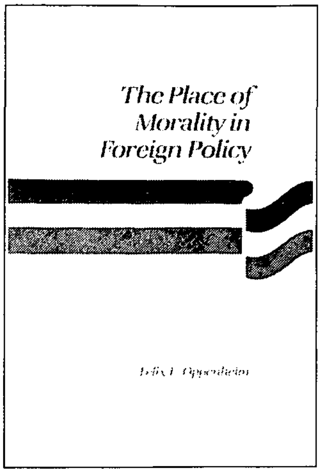

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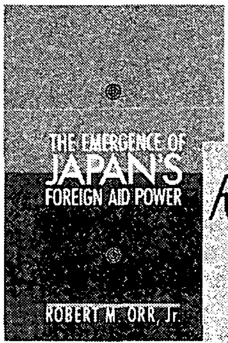
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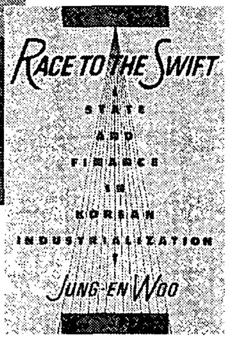
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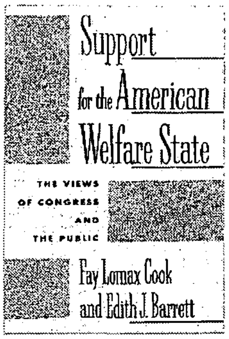
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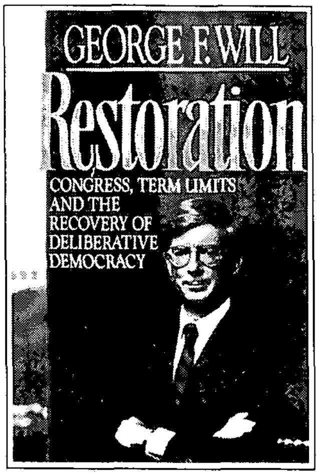
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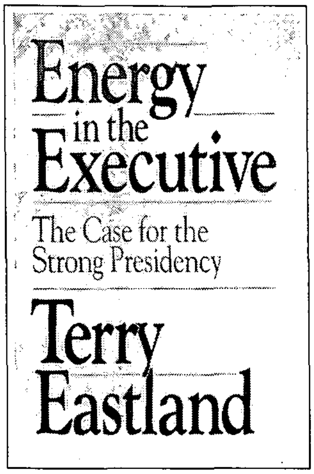

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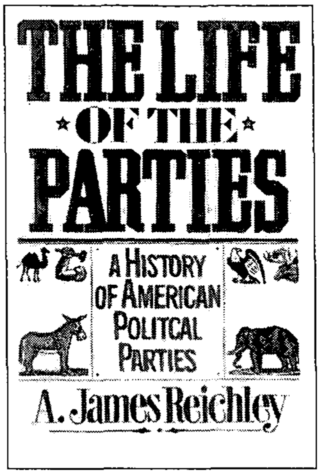

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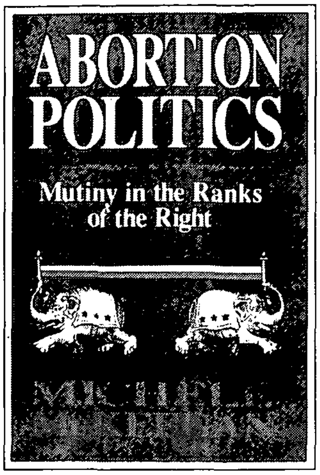

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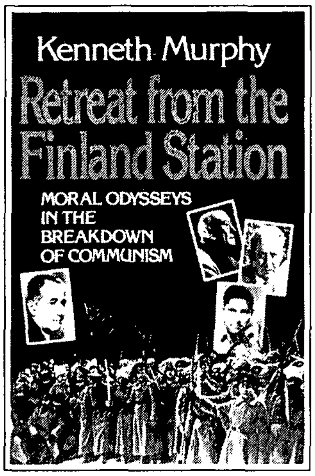
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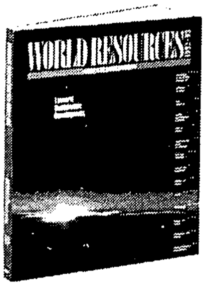

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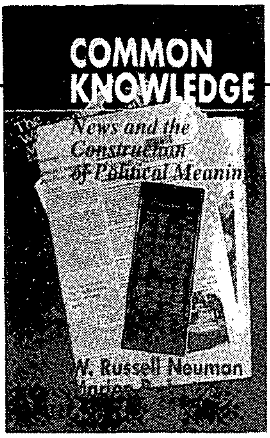

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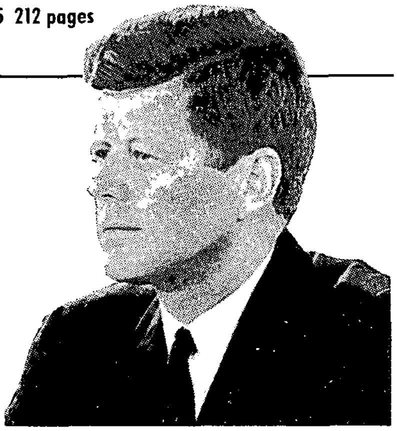

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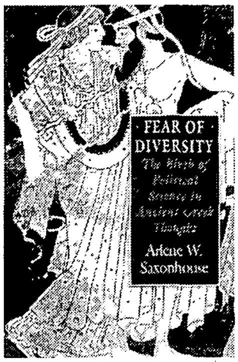

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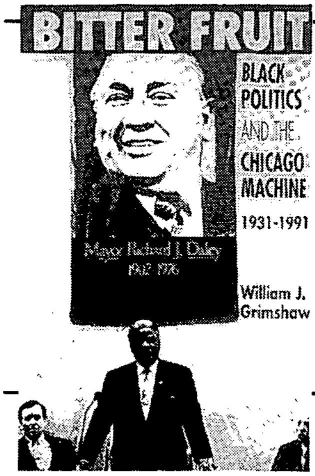

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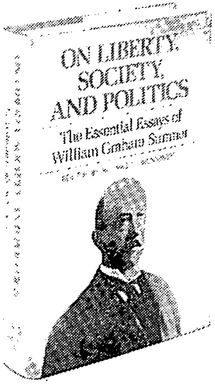

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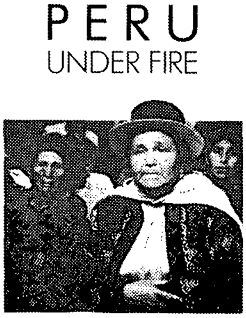

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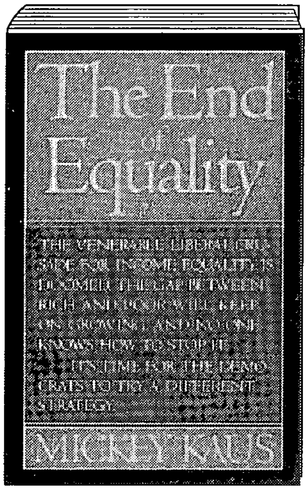
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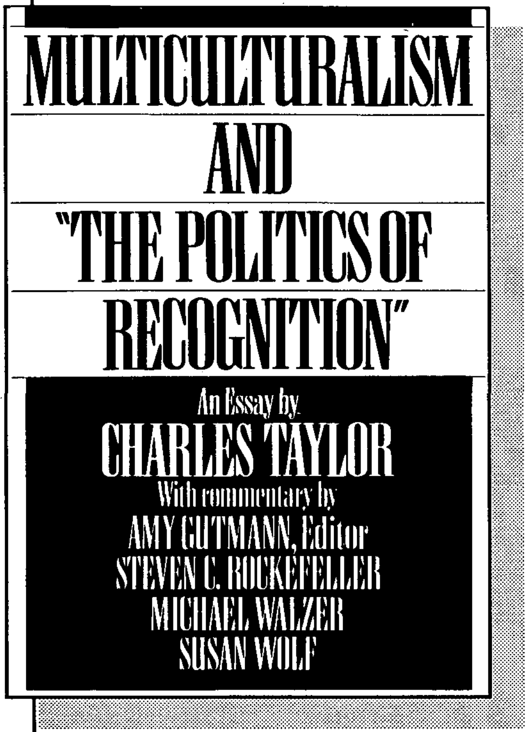

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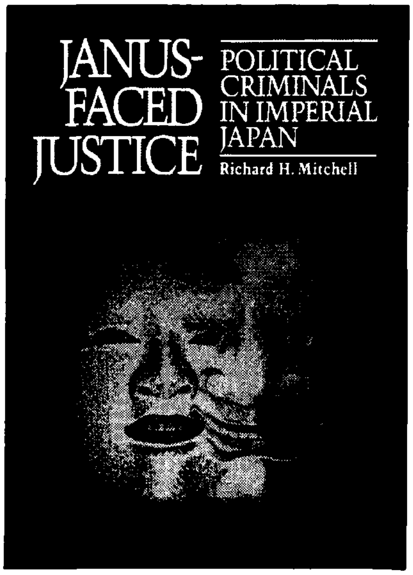

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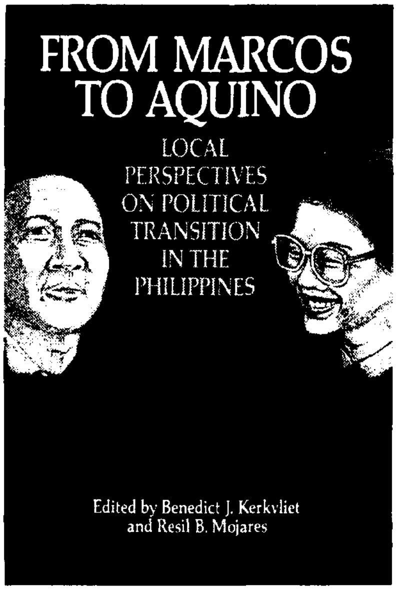
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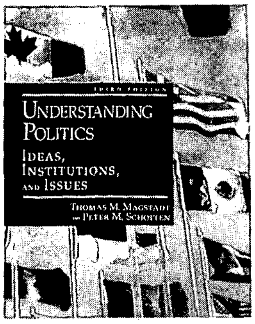

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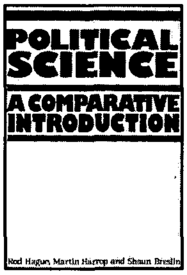

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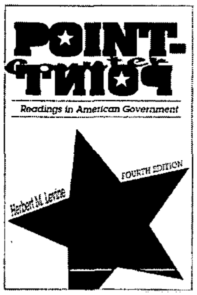

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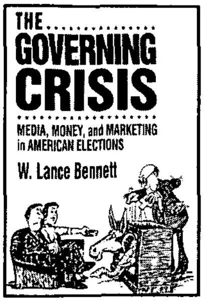

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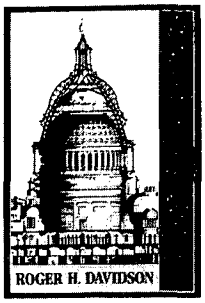

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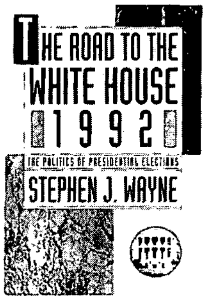
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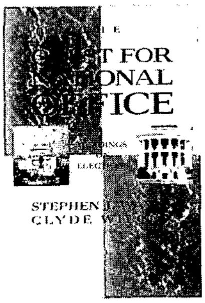

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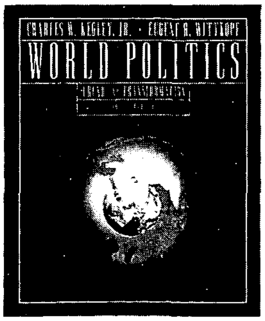

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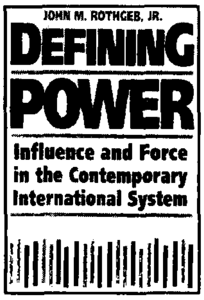
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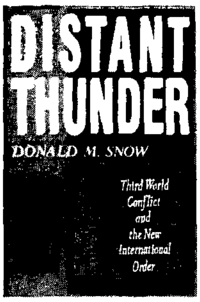
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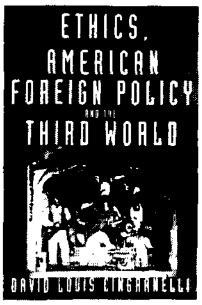
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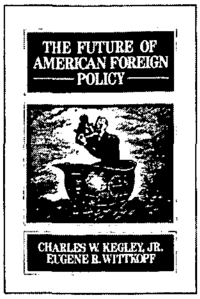
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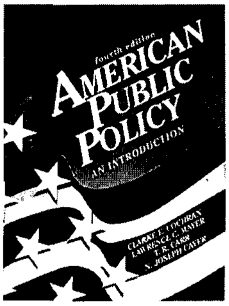
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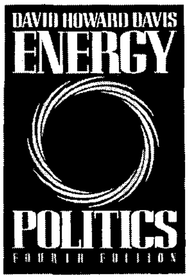
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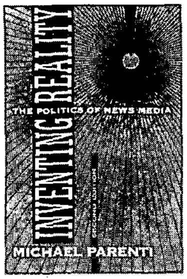

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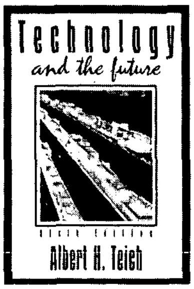

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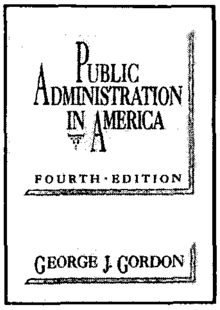

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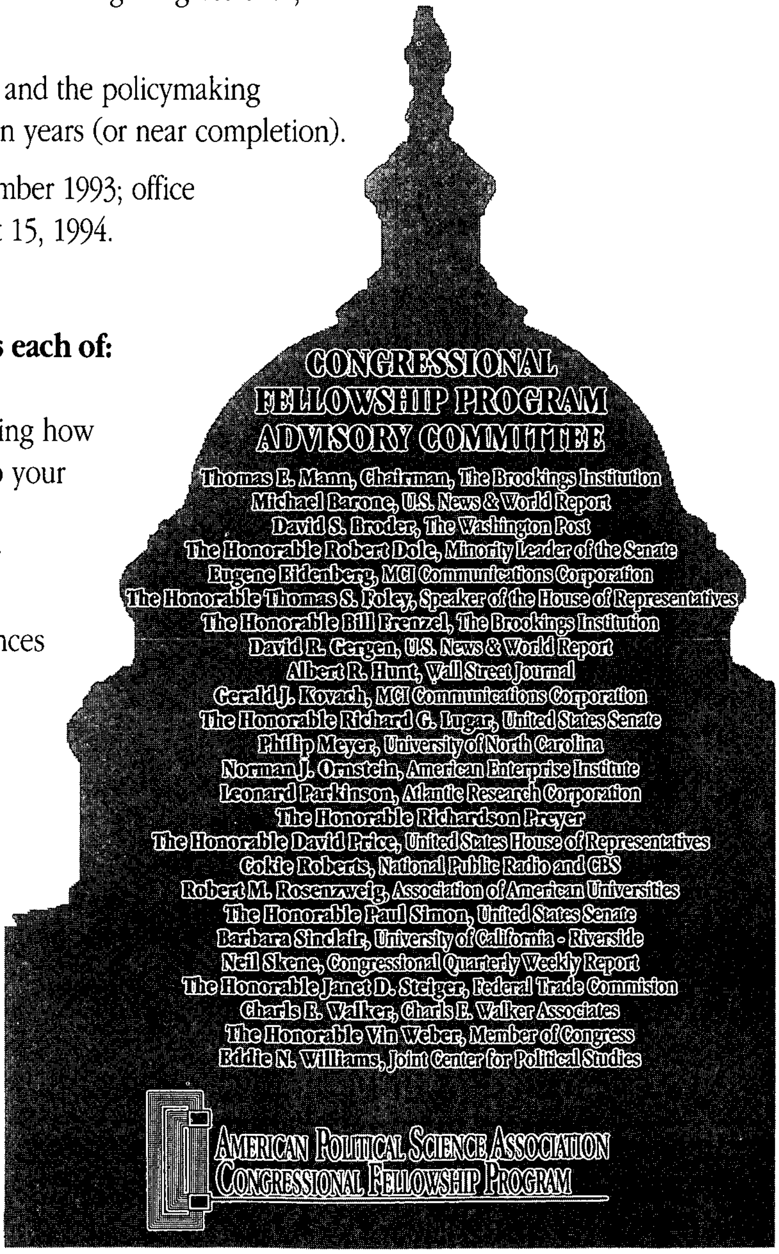

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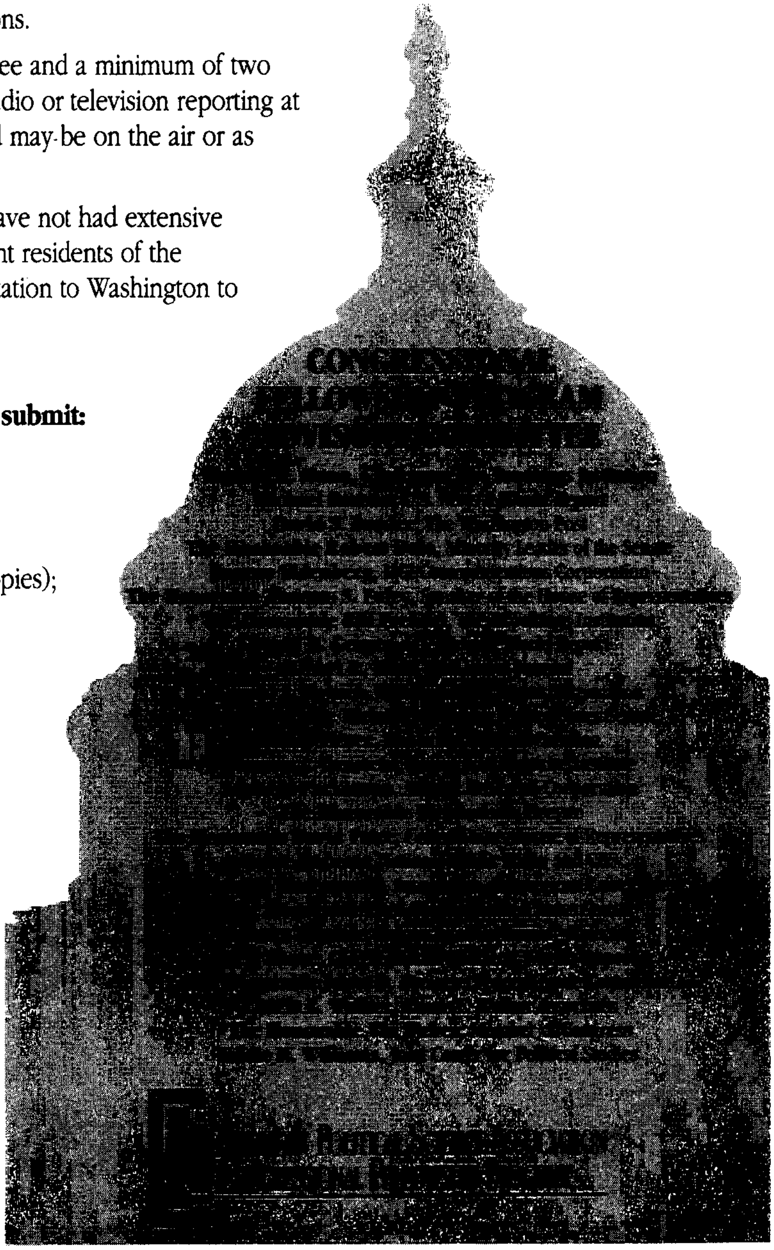

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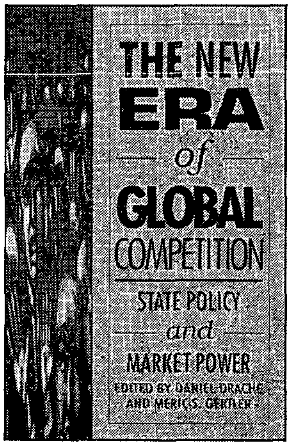

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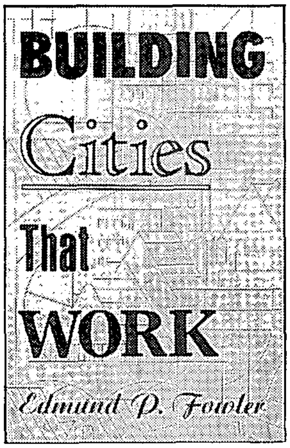

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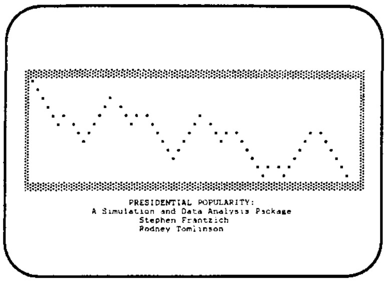

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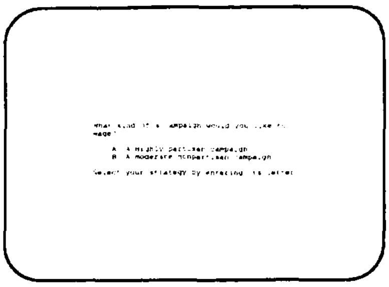

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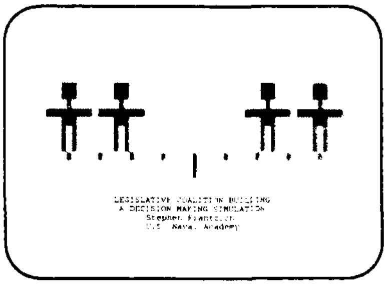

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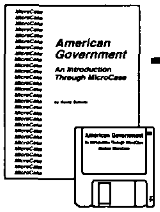

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IS THE CURRICULUM BIASED?

A STATEMENT OF THE NATIONAL ASSOCIATION OF SCHOLARS

American higher education is facing widespread demands to eliminate the allegedly "Eurocentric" and "patriarchal" bias of the curriculum. While the details vary from campus to campus, these demands tend to focus on four objectives:

- that the "canon" be revised to include more works by blacks, other ethnic minorities, and women
- that the "issues of race, gender, and class" be introduced into a greater variety of courses
- that more courses in women's studies and minority studies be developed
- that courses in women's studies and/or minority studies be required of all undergraduates

Various justifications are commonly proffered for making these changes. It is alleged that:

- works by minorities, women, and Third World authors have been excluded from the curriculum
- minority and female students feel alienated and their educational progress is retarded by being asked to study works primarily by white males
- in order to overcome their own prejudices, white males must become acquainted with the cultures and problems of minorities and with the perspectives and problems of women
- the traditional curriculum represents the hegemony of Western culture, covertly supports a status quo inherently oppressive of women and minorities, and is unfairly imposed on students from different cultures
- the traditional desiderata of truth, objectivity, and critical intelligence can be met only by adding the perspectives of women and minorities and by facing up to the new questions they raise
- an increasingly diverse society and interdependent world require that our citizens gain greater understanding of different cultures

The National Association of Scholars disputes the first five of these arguments and believes that the last entails something other than the changes being proposed.

First, any work, whether formerly neglected or widely known, should be added, retained, or removed from the curriculum on the basis of its conformance to generally applicable intellectual and aesthetic standards. A sound curriculum cannot be built by replacing those standards with the principle of proportional representation of authors, classified ethnically, biologically, or geographically.

Second, the idea that students will be discouraged by not encountering more works by members of their own race, sex, or ethnic group, even were it substantiated, would not justify adding inferior works. Such paternalism conveys a message opposite to the one desired.

Third, other cultures, minority subcultures, and social problems have long been studied in the liberal arts curriculum in such established disciplines as history, literature, comparative religion, economics, political science, anthropology, and sociology. But more important, mere acquaintance with differences does not guarantee tolerance, an ideal Western in origin and fostered by knowledge of what is common to us all.

Fourth, the idea that the traditional curriculum "excludes" the contributions of all but males of European descent is patently false. From their beginnings, Western art and science have drawn upon the achievements of non-Western societies and since have been absorbed and further enriched by peoples around the globe. That the liberal arts oppress minorities and women is yet more ludicrous. Even if the curriculum were confined to thought strictly European in origin, it would still present a rich variety of conflicting ideas, including the very concepts of equality and freedom from oppression invoked by those who would reorient the curriculum.

Fifth, while diversity of background is valuable to the discussion of issues to which those differences are germane, objectivity is in general not enhanced but subverted by the idea that people of different sexes, races, or ethnic backgrounds necessarily see things differently. The assertion that cognition is determined by group membership is itself an example of stereotypic thinking which undermines the possibility of a true community of discourse.

Sixth, the study of the traditions and achievements of other nations and of ethnic subcultures is important and should be encouraged. But this must proceed in a manner that is intellectually honest and does not serve as a pretext for inserting polemics into the curriculum. Furthermore, "multicultural education" should not take place at the expense of studies that transcend cultural differences: the truths of mathematics, the sciences, history, and so on, are not different for people of different races, sexes, or cultures, and for that reason alone their study is liberating. Nor should we further attenuate the study of the traditions of the West. Not only is knowledge of those traditions essential for any evaluation of our own institutions, it is increasingly relevant to our understanding of other nations, which, in striking testament to the universality of the values they embody, are rapidly adapting Western practices to their own situations.

The **National Association of Scholars** is in favor of ethnic studies, the study of non-Western cultures, and the study of the special problems of women and minorities in our society, but it opposes subordinating entire humanities and social science curricula to such studies and it views with alarm their growing politicization. Efforts purportedly made to introduce "other points of view" and "pluralism" often seem in fact designed to restrict attention to a narrow set of issues, tendentiously defined. An examination of many women's studies and minority studies courses and programs discloses little study of other cultures and much excoriation of our society for its alleged oppression of women, blacks, and others. The banner of "cultural diversity" is apparently being raised by some whose paramount interest actually lies in attacking the West and its institutions.

We urge our colleagues to demand clear explanations and cogent arguments in support of the proposals being so rapidly brought before them, and to reject any that cannot be justified. The curriculum is and should be open to change, but we must rebut the false charges being made against existing disciplines. We must also reject the allegations of "racism" and "sexism" that are frequently leveled against honest critics of the new proposals, and which only have the effect of stifling much-needed debate.

For copies of this or other NAS statements, or for additional information about the **National Association of Scholars** and its activities, including its fellows program, research center, speakers bureau, search service, newsletter, state and regional affiliates, conferences, local events, and the quarterly *Academic Questions*, write to the **National Association of Scholars, 575 Ewing Street, Princeton, NJ 08540**, or call **609-683-7878.**

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